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MEDICAL AND PHYSIOLOGICAL
PROBLEMS ;
BEING CHIEFLY RESEARCHES
FOR
CORRECT PRINCIPLES OF TREATMENT
IN DISPUTED POINTS
OF MEDICAL PRACTICE,

BY
WILLIAM GRIFFIN, M.D.

AND BY
DANIEL GRIFFIN, M.D.



THE FOLLOWING PROBLEMS :

- I.—What principles should be kept in view in the treatment of enteritis ?
- I.—How are nervous affections distinguishable from inflammatory ?
- III.—Under what circumstances and to what extent is bleeding proper in diseases of the brain ?
- IV.—On what morbid state does the occurrence of coma and sudden death in jaundice depend ?
- V.—Is the law of visible direction, as at present received, a true one ?
- VI.—Is laryngismus stridulus, or the crowing disease, a spasmodic or paralytic affection ?

LIMERICK :
CORNELIUS O'BRIEN, 108, GEORGE-STREET.

1839.

Price Three Shillings and Sixpence.



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BY
WILLIAM GRIFFIN, M.D.

*Member of the Royal College of Surgeons in Edinburgh, one of the Physicians
to the County of Limerick Infirmary, Consulting Physician
to the Lying-In Hospital, &c., &c.*

AND BY

DANIEL GRIFFIN, M.D.

*Member of the Royal College of Surgeons in London, Assistant Physician to the
County of Limerick Infirmary, one of the Physicians to the
Lying-In Hospital, &c., &c.*

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PREFACE.

IN all diseases there is a right as well as a wrong mode of treatment; and it is popularly believed that the science of Medicine has long since determined between the two—that in every dangerous case, or, at all events, in those of ordinary or frequent occurrence, the practitioner has only to refer to received principles or authorities on the subject, and that if he commits an error in his selection of remedies, it is entirely attributable to his want of information or ability. It would, no doubt, astonish the public very much, and do little credit to medical art, if they were told, that there is scarcely a complaint to which humanity is liable, the treatment of which is, in all points, absolutely agreed upon by the profession; and that with reference to the most ordinary of dangerous cases, and those supposed to be best understood, remedies of a different and even opposite nature have been advocated by men of the highest celebrity. It is little imagined the deep responsibility that rests with the young physician, when a decision is demanded of him on such occasions, or the profound judgment, as well as extensive information required to arrive at a correct conclusion between conflicting authorities of equal consideration. Whatever his decision may be, if he is himself sufficiently informed of the difficulties, he must act under the depressing assurance, that if the result is fortunate the judgment evinced in overcoming them will never be appreciated, while, if it be otherwise, there will not be wanting persons to inquire why other modes of treatment were not adopted. It occurred to me some years since, that it would be a most useful, practical and interesting study to collect cases with reference to all the most important of the disputed points in practice, and afterwards as a sufficient degree of personal experience happened to be attained on any

one of them, to review the opinions of all the best authors on the subject, and endeavour to solve that most difficult question—What is the correct principle of treatment? The following papers which my brother and I have contributed from time to time to the medical periodicals, are the results of these reflections. They are now collected for the convenience of easier reference, when it is an object to ascertain the value of opposing doctrines on the treatment of diseases yet unsettled. Some physiological problems have also been discussed, the interest of which may atone for their want of any very obvious practical value.

W. G.

MEDICAL AND PHYSIOLOGICAL PROBLEMS.

PROBLEM I.

BY WILLIAM GRIFFIN, M.D.

WHAT PRINCIPLES SHOULD BE KEPT IN VIEW BY THE PHYSICIAN IN THE TREATMENT OF ENTERITIS.

AFTER the great accessions which have been made to our knowledge of the pathology of the alimentary canal, within the last few years, it may appear extraordinary that I should propose the foregoing as a question difficult of solution. In some points in the treatment of intestinal inflammation, there is, I am aware, a very general agreement among medical men, but in others, and by far the most important, I am far from imagining the practice is at all settled or uniform; there is, indeed, among persons of considerable reputation not only a difference, but a direct opposition of opinion, which sometimes occasions a difficulty in the management of such cases, only conceivable in its full extent by those who have had to stand at the bed side, and choose between the two, with the conviction that life or death hung upon the decision. But even if the practice was more uniform, until some general principles of treatment are universally admitted, there will always be ground for perplexity in the timing of remedies, in determining the changes which are to give us our indications, and in fixing the moment when it will be safe to commence or abandon a particular plan of management. Perhaps much of the discrepancy of opinion that exists might be explained by the success or ill success of remedies under the very varying conditions of the disease in which they have been administered. At all events it would be rendering science some service, if even an approach to truth could be attained, and this is the utmost I

would hope for, until the records of such cases as I shall here offer to the reader's attention, and of those treated on an opposite plan are multiplied and compared.

One might suppose, that at least with respect to the most important of our remedies in all inflammations—blood-letting, there must be a perfect agreement among medical men as to its application in enteritis. Perhaps this agreement may be said to exist within the first twenty-four or thirty-six hours from the commencement of the complaint, but certainly not afterwards.—Dr. Parr—no light authority even at the present day—says, “In the treatment of enteritis there is much doubt. We are ordered to bleed freely, though the pulse is small, and to repeat it till the pulse rises. When the patient is a robust countryman, and the disease induced by drinking cold fluids in a heated state, this advice may be useful, but it is not generally so: Perhaps bleeding is more seldom necessary in this disease than in any other inflammation; for it rapidly tends to mortification, and should it not at once relieve, it soon proves fatal.”

Dr. Mason Good, whose work may be regarded as a compendium of the practice of some of the first physicians in this and other countries, and whose learning and ability must necessarily attach considerable weight to any opinion he supports, speaks of bleeding in much the same qualified manner with Dr. Parr, and says, if it does not succeed, it will assuredly hasten the stage of gangrene, and abbreviate the term of remedial operations. And again, he says, unless the constitution is tolerably vigorous, and the lancet is employed early, there is no inflammation in which the latter is less likely to be serviceable, or may become more mischievous. Dr. Abercrombie has none of these qualifications; he of course admits, as every one must, that bleeding in inflammation of any vital organ, to be of much service, should be used at an early period; but he does not seem to apprehend that it can be readily over done in the first instance, or that its repetition in the advance of the disease is useless, much less mischievous. He recommends after a first full bleeding, smaller ones every hour or two, until the force of the attack is broken; and seems to consider, that the repetition must at any period be directed by the degree of inflammatory action and strength of the patient's constitution, without reference to its supposed effect in accelerating the approach of gangrene. Dr. Elliotson says, “the first thing one has to do is to bleed the patient well; you should set

him as upright as he can be, and bleed from a large orifice without any mercy. You should of course consider the patient's strength; but you should bleed on till you make a decided impression—till you knock down the pulse, and make him faint." These are discrepancies among respectable authorities, sufficiently alarming at the very outset, but they fall far short of others, which the inexperienced physician will find himself surrounded by, at his next step in the treatment—the administration or rejection of purgatives.

To revert to Dr. Parr again, he says: "The salutary termination of the disease is by a discharge of fæces; if this can be obtained the patient is safe." Should mild purgatives prove ineffectual, he recommends that the more acrid ones be resorted to, and gives the same advice respecting injections. Dr. Pemberton, after premising general and local blood-letting, recommends, "if the stomach will bear liquids of any sort, a strong solution of epsom salts in mint water, with an addition of tincture of senna in such quantities, and at such intervals, as the sickness of stomach will allow. If, however, all liquids are rejected, we may direct an usual dose of calomel in union with the compound extract of colocynth every six hours, *ad quartam vicem*. In the intermediate hours, an injection of water-gruel with common salt may be employed. Purgatives are to be continued during the whole course of the complaint." Dr. Mason Good says, "from the first we must attempt cathartics, and if the stomach will not retain the milder, we must have recourse to the more acrid." He asserts, "it does not necessarily follow, that the irritation of those more acrid purgatives will add to the inflammatory irritation, and that the cure depends almost entirely on our success in procuring free evacuations." Dr. Elliotson says, "after bleeding, undoubtedly a very large dose of calomel should be exhibited; after it has been taken some time, other purgatives should be given with active injections, so that if possible they may meet half way and combine, and then," as he expressively asserts, "*out goes the disease*." "We should first," he tells us, "bleed freely, because purgatives will not operate until we have done that; we should then give a large dose of calomel, such as a scruple, by the mouth, and a strong purgative injection, with plenty of salts, or salts and senna, or colocynth, or oil of turpentine, and repeat the calomel in ten grain doses every four or six hours, *giving purgatives in addition from time to time*."

Let us contrast these with other opinions of the very highest character. Dr. Abercrombie says, "we have seen the bowels obstinately obstructed, and we have seen them spontaneously open or easily regulated, and in both cases the disease has run its course with equal rapidity to a fatal termination. We have found no reason to believe that the retention of fæces was in itself injurious in one case, or the free evacuation of them beneficial in the other; on the contrary, we have had evident reason to believe, *that in several cases in which the inflammation appeared to be subdued, the action of a purgative was immediately followed by a renewal of the symptoms.* Along with these considerations, we must keep in mind the fact, that in the ordinary cases of enteritis, the action of purgatives is in general entirely fruitless; they are usually vomitted as often as they are given, and consequently can only prove additional sources of irritation. I know that much difference of opinion exists upon this subject among practical men; but upon the grounds now referred to, *I confess my own impression distinctly to be, that the use of purgatives makes no part of the early treatment of enteritis; on the contrary, they are rather likely to be hurtful until the inflammation has been subdued.* When we have reason to believe this has taken place, the mildest medicines or injections will often be found to have the effect, after the most active purgatives had previously been given in vain. In the general treatment of enteritis indeed it is desirable to keep the bowels if possible free from distention; but this object may, I think, be usually obtained by mild injections, or by the tobacco injection." Dr. Gregory, of London, and other physicians of equal respectability, entertain similar views with Dr. Abercrombie in the treatment of this disease. Broussais limits the use of purgatives to the close of the acute stage, and Armstrong, of whose practice I shall have to speak again presently, leaves them altogether out of sight, or mentions them only as requisite when the inflammation is over. It remains for us amidst these conflicting opinions to inquire anxiously on which we are to rely in practice. The following interesting case may assist us in determining the question.

CASE I.—A lady, aged thirty-two, while engaged with an evening party, on the 1st of November, was affected with pain in the lower part of the back, and great weariness. She took three glasses of port wine with the hope of relieving it, but they did her no service; she became feverish in the night, and in the

morning (2d) had headache and thirst with a pulse at 90; she was ordered some castor oil. At three in the afternoon she complained of severe pain in the stomach, and on examination the whole of the abdomen was found excessively tender to the touch, and somewhat full; she was warm and restless, and had an anxious and painful expression about the brow. *The castor oil had operated two or three times freely.* As I could not detect any tenderness of the lumbar spine, although she still felt the pain about the sacrum, I was satisfied the attack was purely inflammatory, and placing her upright in the bed, I opened a vein in the arm. Eight ounces of blood were hardly drawn when she fainted; the orifice was closed; she got some warm drink, and was laid in the horizontal position until she had perfectly recovered. The orifice was then re-opened, keeping her in the recumbent posture, but after losing two ounces more, she fainted again. Finding it impossible to procure the desirable quantity of blood in this way, I applied two dozen leeches, chiefly to the right hypochondriac and iliac regions, which were more exquisitely tender than other parts of the abdomen, and gave three grains of calomel and a grain and a half of opium, with two grains of the former, and one of the latter, every second hour. The leeches drew about eight ounces of blood, and a great deal drained away through the remainder of the night after they fell off. In the morning (the 3rd) the pain still continued extremely severe, and the tenderness was so acute, that she could not turn to either side, and scarcely could bear the bed-clothes to rest on her; it extended over the whole of the abdomen. I now took twenty ounces of blood from the arm freely, and without producing faintness. As the bowels had not been moved since they were affected by the castor oil in the middle of the day before, and there was some tumidity of the abdomen, pills composed of equal parts of aloes and extract of henbane, were ordered, and an oatmeal tea injection with castor oil, and two teaspoonfuls of spirits of turpentine. On administering this last she was seized with a dreadful forcing or bearing down pain in the rectum, and passed nothing; the pain seemed as excruciating as any that could occur in violent labour, lasted for about twenty minutes, and was then relieved by the warm bath. In two hours afterwards a simple injection of oatmeal tea was given, followed by similar suffering, and was in like manner retained. The permanent pain was at this period severest in the left iliac region

and about the navel, where the tenderness on pressure was extreme; the countenance was more anxious; the tumidity of the abdomen was increasing, and the stomach beginning to reject the drink. In consultation with another physician, it was now agreed to take blood again, and eighteen ounces more were drawn, being the third general bleeding within twenty-four hours. Two grains of opium and a grain of calomel were given immediately after, and ordered to be repeated every two hours through the night. In the morning (the 4th) there was a considerable improvement; the abdominal tenderness was diminished, the pain and sickness of the stomach had very much subsided, and the injections had come away with some dark, thin, feculent matter; she still, however, felt pain and a sense of great weariness at the lower end of the sacrum, shooting up through her back; and she had a great difficulty in passing water. She now informed me, that a few days previous to her present illness, she was attacked with a profuse leucorrhoeal discharge, attended by heat and sense of scalding, but that it had since abated or almost ceased. A fomentation to the lower part of the abdomen was ordered, and the opium was continued in two grain doses, every two hours, without the calomel. In the evening the improvement appeared progressive; the skin was cool; the pulse soft at one hundred and ten; the tongue cleaner; the abdomen was still full, but had nearly lost its tenderness, and she could turn in the bed with little pain. She spent the night and the following day (the 5th) with little uneasiness except for the soreness in the lower part of the back, and difficulty in passing water, which sometimes occasioned violent straining and bearing down.—Though suffering much in this way, she would not permit the removal of the water by the catheter. She now for the first time asserted, that matter was passing from the rectum; it was, however, supposed to be merely a return of the leucorrhoeal discharge; no further attention was paid to it; and as there were so many other symptoms of amendment, the opium was ordered to be given in grain doses only, every fourth hour. She had already taken thirty-two grains within the last thirty-two hours, without its occasioning stupor, headache, or any other unpleasant symptom, except on the last night, when she complained of frightful dreams and startings. On the next evening, as she lay on the sofa while her bed was making, she felt a solid substance passing from the rectum, which alarmed her terribly. It was

found to be a rope of sloughy stuff, soft and purulent outside, but tough and fibrous within, not unlike the ischiatic nerve in a decayed state, suspended from the rectum for the length of a foot or more. On attempting to draw it away, it appeared to be still adherent within the gut, and she complained of pain. After a little, however, it was removed without much effort, and a gush of matter to the amount of perhaps two tablespoonfuls followed. The slough was about the thickness of the thumb, or more, and was fifteen or sixteen inches in length. We at first supposed it was a portion of the small intestine which had mortified, and been thrown off; but on close inspection no distinct traces of a canal could be found. Sometime after an injection of warm water and sweet-oil was administered, which came away in about twenty minutes mixed with some matter, but without any appearance of feces. On examining the rectum, a rugged irregular edge was felt at the posterior side, close to the sacrum, as if it was the termination of the part from which the slough had been cast off; the examination gave much pain, especially when the intestine was pressed upon within. Several days passed without much alteration in the case, there was matter daily discharged to the amount of three or four ounces, and there was at times severe dysury, at last demanding the use of the catheter. The urethra was blocked up with a thick mucous discharge, which closed the common instruments, and prevented the water escaping until a very large sized flexible one was introduced. Sometimes the difficulty appeared to be connected with mere nervous irritation, as she occasionally got sudden and unaccountable relief, the water coming off without any very obvious reason. Three days had now elapsed since the subsidence of the pain and tenderness of abdomen, *and six days since the bowels had been moved.*—There appeared to be some fulness of the abdomen, and she began to feel uneasiness again in the left iliac region; we had allowed the bowels to remain so long at rest with a view to the healing of the ulceration in the rectum, and from an apprehension that much disturbance of the intestine might increase the inflammation and sloughing. It was evident however, that there might be considerable risk in allowing the distention to go on further. A dose of castor oil was therefore given, followed by pills of aloes and henbane, every second hour, which operated freely in the course of the day (November the 8th); the motions were thin, dark, and streaked

with matter on the surface. The pain in the left iliac region nevertheless, to our great surprize and alarm, became progressively worse, and extended rapidly over the whole of the abdomen; there was a return of the restlessness and distress of countenance, and the stomach began to reject every thing. The case now presented a more alarming aspect than it did even on the former attack; there was increasing distention of the abdomen, the pulse became feeble and rapid, the thirst extreme, the vomiting frequent, the countenance was sunk, the look anxious, and the face was covered with clammy perspiration. We were here placed in one of those difficult situations in which the diversity of medical opinions on the treatment is most distressing to the practitioner; after all his reading and study of the subject, throwing him back without clue or guide upon his own resources. There was too much debility, and the complaint was too far advanced to venture on general blood-letting; leeching alone could effect little; and purgatives, which operated freely both at the commencement of the first attack and of the present, not only without relief, but with apparent disadvantage, might only increase irritation and render recovery hopeless. The recurrence of the inflammation must have arisen either from the slough having penetrated the intestine and allowed its contents to escape into the cavity of the abdomen, or from the direct effect of the purgative; or it might be from our having deferred it too long, and permitted the bowels to become injuriously distended. We thought it exceedingly probable, from the extent and depth of the slough at the time it was cast off, that perforation would eventually take place, and as that event is not always announced by the sudden pain usually ascribed to it, there was reason for strong suspicion of its having occurred. On the other hand, the fact that inflammation of the bowels has often recurred from the imprudent administration of purgatives,* and that in this case it had only increased after free evacuations, gave some colour to our apprehensions that they had done mischief.—The argument was, after all, perhaps equally strong in favour of the third and opposite proposition, that the distention of bowels had been allowed to go on to an injurious extent. We had determined, in fact, so great was our apprehension of increasing the slough, and thus occasioning perforation of the intestine, not to give a purgative until we had some manifest indication that the

* Abererombie furnishes instances of this, and more lately Dr. Stokes, of Dublin.—See Dublin Medical Journal, vol. i. p. 128.

confinement of the canal was doing harm, and hence necessarily the uneasiness of bowels commenced before the purgatives were given. All, therefore, that could be fairly inferred from the continuance and increase of the inflammation after their administration, was, that they had failed to arrest it. The result of these considerations was, our trusting to opiates (which had been so successful before) for removing the inflammation, and our determining to watch the progress of the case, that we might, if possible, detect the real cause of its recurrence. Three grains of opium were given at first, and two every second hour afterwards : a dozen leeches applied to the abdomen, and fomentations, with decoction of poppy heads, were made use of. The effect was wonderful ; the pain and tenderness gradually subsided, the vomiting ceased, the pulse became slower, and she got some sleep. In the morning (November the 9th) the improvement was more considerable ; the pulse fell to 110 ; she retained every thing on her stomach, and her countenance was full of hope and cheerfulness ; the tenderness of the abdomen had almost entirely subsided. This perfect relief without the use of any purgative was little calculated to relieve us from our perplexity as to future treatment. She made little complaint for the ensuing five or six days, except that she was occasionally annoyed with dysury ; and the continual passing of matter from the rectum, with pain and soreness inside the sacrum, as she described it, which made it distressing to her to cough, or laugh, or sit up in the bed to take food. After the great amendment on the 9th, the opium was gradually diminished, at first to a grain every third hour, and eventually to a grain and a half or two grains at bedtime only. The appetite had returned, the tongue was clean, and she took a fair quantity of nourishment daily, chiefly gruel or steeped bread. We now watched the state of the abdomen with much anxiety, and especially about the left iliac region, where the pain commenced before, and where indeed the injurious effects of distention were likely to be first felt, as in all probability the sloughing extended as far as the sigmoid flexure of the colon. There was already a considerable fulness, but as there was no tenderness whatsoever on pressure, and as we had just a chance that a natural motion might take place, we still refrained from giving a purgative. About the seventh day from that on which the bowels were last moved, the tenderness in the left ilium, as we anticipated, was again felt, and it was soon fol-

lowed by pain and feverishness, with a disposition to vomit.— There was now no doubt on our minds that the recurrence of the attack was attributable to distention, and not to perforation of the intestine, as we had apprehended. After giving a large opiate, therefore, she was ordered a few grains of calomel, with mild doses of castor oil and jalap every second hour, until the bowels were freely moved. Great relief was obtained, but the pain and tenderness of the abdomen finally subsided on resuming the opiates for twelve or fourteen hours after the purgative had ceased operating.

Although she was now for the third time freed from all symptoms of active inflammation, we could not but consider her situation as very precarious. One should be very sanguine to look forward with any confidence to the healing of such an extensive sore as remained after the detachment of a slough of fifteen or sixteen inches in length, and from which still matter was daily discharged, while the most minute ulcerations of the rectum are so troublesome and difficult of cure. While it was considered an object, under these circumstances, to keep the bowels as long as one safely could without acting, care was taken that no injurious accumulation should again occur. A mild purgative was given at the farthest on every fourth day, which operated without creating pain or uneasiness, and by diminishing the interval gradually, the bowels were after a little brought to act daily with a small dose of infusion of rhubarb and cascarrilla. She was still however unable to sit up in the bed, or to turn to either side on account of the excessive soreness inside the sacrum: the motions continued to be smeared with matter; sometimes small bits of fresh slough came away; sometimes spoonfuls of healthy pus with stuff like jelly: weak sulphate of zinc injections, and even those of simple water, were made use of, but they gave great uneasiness, and served to do more harm than good. At this time, about eight weeks from the commencement of the attack, she became very hysterical; got fits of crying and laughter, which lasted for hours, and was sometimes slightly delirious. She had been kept very low althrough her illness, but was now allowed nourishing diet, meat and a little wine: there was an immediate improvement in all the symptoms; her strength and health mended; her mind became cheerful; the discharge of matter diminished, and at last was only occasionally observable. The soreness about the sacrum was also lessened so considerably, that

she was able to dress, lie on the sofa, and sometimes sit up for a short time. At the end of three months she could move about the room a little, and at the termination of the fourth, she was perfectly recovered, and able to walk some miles in the country without injury or fatigue.

The occurrence of sloughing of the rectum, and perhaps of the colon in the foregoing interesting case, to an extent I believe unparalleled in the records of medicine, created much embarrassment and difficulty in its management. The treatment cannot therefore be reviewed as applying to a case of simple enteritis, without reference to the incidental dangers with which it was complicated. The reader's attention may be fairly directed to some very important facts, and the inferences deducible from them, which bear distinctly upon the general principles of cure in that complaint, and first of blood-letting.

I shall not waste time in debating whether this remedy is advantageous in the early stage of enteric inflammation, any more than I should in a similar affection of any other viscus. It is, I believe, universally admitted at the present day, that blood-letting and the amount to which it may be carried, should bear reference only to the strength of the patient : it is also the practice to estimate the strength by the state of the patient's health previous to the attack, and not at its commencement. A strong man, for instance, may be seized with enteritis, the powers of life may be suddenly depressed, and the pulse become feeble : This is obviously a state of indirect debility ; there is no outgoing ; nothing in the time to account for it ; the man will bear bleeding well, and become stronger as the blood is taken away from him. On the other hand, a strong woman may become exhausted in a tedious labour ; she may be delivered with instruments ; she may get peritoneal inflammation. Here if we find a very feeble rapid pulse and other appearances of debility, we cannot with the same readiness pronounce it to be indirect ; for the patient has been probably worn out before the inflammation began, would sink under a large bleeding, and must be treated by other means. In the case before us, it will be observed, that the lady fainted at the first bleeding, when eight ounces were drawn, and so little could be obtained afterwards by the lancet, that leeches were resorted to ; yet the inflammation went on, and next morning twenty ounces were taken from the arm, and on the same evening eighteen more, without inducing the slightest symptom of weakness.

The debility in the first instance was evidently indirect, and probably depended on some nervous idiosyncrasy. Occurrences of this kind would lead one to doubt the propriety of always placing the patient upright in the bed, as Dr. Elliotson directs in taking blood. His object is to insure its producing some effect on the arterial system ; to bring on syncope : but it is evident, it may bring it on sooner than it would be at all desirable in habits particularly constituted. When syncope occurs without any direct relation to the loss of blood, the effect may be of use in colic ; but in violent inflammation it is temporary only ; as soon as the heart recovers itself it has yet all the material for maintaining its action strongly, and the inflammation consequently revives with it. There must be an absolute abstraction of blood capable of directly depressing the whole system to produce any permanent influence on the disease. Dr. Marshall Hall's ingenious conjecture, that the amount of inflammatory action might be estimated by the power the system evinces of resisting syncope in the erect position, is only true in the advanced stages of inflammation, when all the idiosyncrasies are sunk or lost in the one great absorbing action. In the commencement the character of the habit prevails ; in the advance, the character of the inflammation only. At first we shall have differences in the symptoms in different individuals, though the disease be the same : by and by these disappear as the inflammation rises, and it displays an identity of symptoms in all. Dr. Marshall Hall's test of inflammation will, therefore, we fear, be often found unavailing when assistance in the diagnosis is most needed ; that is, in the beginning. In the case, the history of which I have detailed, the lady, though healthy and of a full person, was of a very hysterical habit, just such a one as those attacks of simulated inflammation might readily arise in. The occurrence of syncope on withdrawing a few ounces of blood, made it still more probable, that it belonged to the class of irritative disorders, rather than to those of a purely inflammatory nature. But on examining the spinal column to ascertain whether there was any morbid state of the cord to which the excessive tenderness and pain of the abdomen could be ascribed—*there was no soreness on pressure*. Notwithstanding the fainting, I had, therefore, no hesitation in looking on the attack as one of severe inflammation, and in this I was amply borne out by the event.

But to return to our subject ; no one now, as I have said

doubts the propriety of bleeding as largely as the strength will allow in the early stage of enteritis, or that the earlier the lancet is made use of the better. But the question remains, shall we not bleed at any period of the disease while the inflammation exists, limited only, as in the early stage, by the powers of the constitution? It is said, and as I have shewn by high authority, that bleeding, if it should not succeed, and its success is of course very doubtful in the advance of the disease, hastens the stage of gangrene; this, it must be admitted, is very contrary to what occurs in the inflammations of other organs, and to the very prevalent belief among medical men, that gangrene in these instances results purely from the violence of vascular action. If the tendency to it arises from a peculiar diathesis existing at the time, bleeding can of course do no good, and may possibly prove injurious; but what remedy in such a case could save the patient. If on the other hand, it supervenes from the violence of the inflammation, what can prevent it if bleeding does not? Though disposed, as far as my own views are concerned, to reason in this manner, I can by no means treat slightly the apprehensions of those able physicians who have condemned the use of blood-letting in protracted enteritis. Their opinions, however erroneous, must have been founded on the experience of its ill success, and before we can fully reconcile ourselves to the practice, this ill success should be accounted for. It will be allowed on all hands, that one large bleeding has often subdued this and other dangerous inflammations, when employed at an early period; while depletion to any amount scarcely succeeds, if it has lasted some ten or twenty hours. There is, then, evidently, even with the same apparent amount of inflammation, a considerable difference in the actual pathological condition of the system at the end of the first, and of the second, or third day; there is some change of state or structure produced capable of keeping up the inflammation, which did not exist within the first twenty-four hours; the inflamed part has acquired a vitality beyond that of other organs, and as bleeding only subdues inflammation by diminishing the vital powers generally, those organs will necessarily die before the inflammation can be thus subdued. It follows, therefore, if the indication with which we commence depletion, that of absolutely subduing the disease, by abstracting from the powers of the system at large, should direct us all through its progress, we should, after a very short period, be aiming at what was

impossible of accomplishment, and very often exhaust the powers of life beyond the hope of recovery. It was this error we apprehend, that occasioned all the ill success which made such an impression on the older practitioners, and brought a most valuable remedy into unmerited disrepute. The truth seems to be, that when we have failed in arresting the inflammation by free and early bleeding, we must give it up as our main resource, and employ it as auxiliary only. We may still make use of it, but it should be simply with the view of diminishing or breaking the violence of the disease, and rendering it more amenable to remedies which operate after a different manner : to leeching, which, by drawing blood from the inflamed parts directly, subdues the disease in a far greater ratio than it subdues the living powers generally : to opiates, which, in diminishing the sensibility of the system, seems also to lessen its power or tendency to support inflammation ; to mercurials, which counteract its action ; or to counter-irritation, which relieves by derivation to some less important tissue. This was the doctrine, or at least the treatment of Armstrong, and that his admirable papers on inflammation have as yet had so little influence on general practice, is to us one of the wonders of modern medicine.

I now come to consider a question of much greater difficulty, and about which practitioners are almost as much divided in opinion at the present, as at any former period ; the employment of purgatives in this disease. It is an undeniable fact that in a great number of cases of enteritis, the salutary termination of the complaint has been by free evacuation from the bowels, and that before this occurs perfect relief is seldom obtained. To the experience of this strong fact, we may readily refer the popularity of the purgative treatment, and indeed, it would seem almost extraordinary, that other and so different modes of management should have made any way in opposition to it. Those instances, however, of occasional occurrence, in which the complaint went on to a fatal termination, although the bowels were free or easily moved, through the whole course of it, or those in which perfect relief was obtained, as in the one I have given, without any discharge at all by the bowels, necessarily startled the practitioner, and led him to inquire whether he had not been too hasty in generalizing his conclusions. Effects are too often, in the science of medicine, mistaken for causes. When cholera

first appeared in this city, calomel was profusely employed in its cure, and it was eventually found, that patients who became salivated almost invariably recovered. This was esteemed proof positive of the efficacy of the treatment, and mercurials became more popular than ever. I found, however, on examining the registries of the hospital with which I was connected, at the termination of a month, that in the stage of collapse no more than one patient in ten could be brought under the influence of mercury, so that there were only four recoveries in forty.*—This told little for the remedy, as far as cases in collapse were concerned, and I immediately set about ascertaining what the amount of spontaneous recoveries might be in the same stage.—From all I could gather from the experience of others or my own, I began to suspect that they would reach nearly the same amount, and at last I became perfectly convinced, that the actual fact was, *the patients did not recover because they were salivated, but they were salivated because they recovered.* Mercury in any shape, in the stage of collapse, was thenceforward discarded from my practice in the hospital, and though it excited some observation at the time, the subsequent experience of the profession at large bore me out in the decision. I cannot but feel, that somewhat of the same error prevails with respect to purgatives in enteritis; the disease is not a very common one, and the experience of an individual could scarcely warrant him in offering opinions at all confidently, when they are opposed to general practice; but certainly all the information I can glean, or the experience which has fallen to my share, would dispose me to say, that in intestinal inflammation, the relief obtained is seldom the direct effect of the purgative, and that *people do not recover because they are purged, but they are purged because they recover.* I shall, however, examine some other arguments that have been offered in favour of this treatment.

“For what reason,” inquires a reviewer of Dr. Abercrombie’s opinions on the subject,† “do we employ active purgatives in the early stage of thoracic inflammation? To lessen the whole

* The same registries proved the decided efficacy of calomel, at any period of cholera previous to collapse. Of those patients who were brought in with the pulse perceptible at the wrist, we sometimes lost but five in the hundred, and never during the most fatal period more than fifteen.

† See Medico-Chirurgical Review for September, 1820.

mass of circulating fluids, and reduce the general action of the heart and vascular system. Now, in abdominal inflammation, provided the mucous tissues are not inflamed, purgative medicines excite the secreting vessels, not only of the whole internal surface of the intestines themselves, but of the glandular organs whose excretory ducts open into the *primæ viæ*, and thus powerfully deplete locally the vascular system of the abdominal viscera." Again it is said, "when that portion of the peritoneum reflected over the intestines is inflamed, and the villous coat unaffected, to excite the natural action of the mucous membrane, immediately after proper vascular depletion, is a powerful means of checking the disease; in the same way, that a free expectoration from the mucous membrane of the lungs, relieves the vascular turgescence and inflammation of the parenchymatous structure or pleural covering of the same organ."

With respect to the first argument it is sufficient to observe, that no one can dispute the utility of the indication, or that purgatives would tend to fulfil it, in any inflammation in which the bowels themselves were not engaged. But it is a different matter attempting to lessen the quantity of the circulating fluids, and diminish vascular action, by stimulating healthy secreting surfaces to action, at a distance from the inflamed parts; and stimulating the inflamed part itself, or parts contiguous to it.—All analogy is against the principle of exciting the action of inflamed tissues; the only exception to which, that I can bring to mind, is to be found in the inflammation of mucous membranes. These, it is ascertained beyond doubt, are often more readily cured by the application of strong stimulants of a particular class after depletion, or sometimes without it, than by the usual antiphlogistic treatment. An ointment of strong nitrate of silver is found to cure the purulent ophthalmia of children and indeed a large proportion of all simple inflammations of the mucous membrane of the eye, more rapidly and permanently than purging and leeching. An injection of the same preparation is said to subdue recent gonorrhœa more speedily than other treatment, and we have indisputable testimony of the advantage derived in dysentery from the use of purgatives, although they have been so deprecated by the Broussaists, and indeed by those of a much less theoretic school. The advocates for purgatives in peritoneal enteritis, adopt this principle, but they adopt it too literally, and without sufficient reference to actual results. They are strictly

for adherence to the rule of leaving inflamed parts at rest, without making any exception in favour of mucous membranes, and their doctrine, consequently, is, to purge when the peritoneal coat only is affected, as in enteritis, not to purge when the mucous coat is the seat of disease, as in diarrhoea or dysentery. In both I differ from them ; I have shewn that inflammation of mucous surfaces cannot be brought precisely within this law, which seems to influence all the other tissues ; and with respect to the propriety of purging in enteritis, because the mucous coat is free, it appears to be introducing a refinement in practice, which the results will not bear us out in. Is it absolutely true that we can violently stimulate a mucous surface which is healthy, in connexion with a muscular or serous which is inflamed, without increasing the inflammatory actions in the latter ? I believe not : on the contrary, to refer again to the eye, where we can see the thing, we find that the same stimulants, that in a state of simple inflammation of the conjunctiva produce a rapid cure, will, if there be any inflammation of the sclerotic coat, make both textures worse. Mr. Guthrie is never in the habit of using the nitrate of silver ointment, when any of the internal coats of the eye are engaged in the inflammation, although if the theory I have been objecting to was correct, stimulating the conjunctiva to increased secretion ought to relieve the sclerotic. It seems, in fact, not only true, that it is injurious to excite parts acutely inflamed into action, but it is not safe to stimulate parts which are contiguous or closely connected with them ; and this is commonly held in view in the very instance which the writer before referred to cites as an analogy, that of inflammation of the lungs or pleura. It is never attempted to stimulate the vessels of the bronchi to increased action, until the inflammation of the lungs is subsiding under the influence of remedies which operate in a different manner, as blood-letting, emetic tartar, calomel and opium or blistering. It is indeed well known, that efforts to bring on expectoration in acute pneumonia previous to large depletion will always fail, while it sets in almost spontaneously if the inflammation is first got under control. In the same manner it is almost universally found, that purgatives will not operate in the early stage of enteritis, unless very free depletion be made use of ; but that when depletion is premised, and the force of the inflammation broken, motions will be found to come on naturally, or with the assistance of the mildest medicines. Thus

what are said to be most important parts of the treatment in both instances, expectorants and purgatives, are absolutely unavailing or injurious when most required ; that is, when the inflammation is at its highest point, and disorganization rapidly proceeding. This cannot be said of the remedies which have been preferred by those who are indisposed to the employment of purgatives in the early stage of enteritis, as leeching, opium, or calomel and opium ; they are useful from the first, and would in numerous instances effect cures without blood-letting at all, or where it had failed, or was totally inadmissible.

To revert again to the case of enteritis, the history of which I have given above, there was an accidental peculiarity in the management, arising out of our apprehension of the sloughing, which led to some interesting results. We not only avoided purging, but for a time designedly kept the bowels at rest, that there might be opportunity given for reparation. If purgatives were so essential, this would have been fatal ; yet in one instance the bowels were unmoved for six days, and in another, seven days, before injurious distention took place. In the third, where they were allowed to remain confined to the fourth day only, no injury at all was sustained. *On all these occasions purgatives had been given at the commencement of the inflammation, long before it had reached an alarming amount, and they had operated freely without giving the slightest relief.* If they had been persevered in, instead of the opium treatment, will any physician take upon him to say, that the termination would not have been fatal ? or that any other remedial plan would have procured such rapid amendment and with such apparent certainty ?

It is not a little in favour of the opium treatment, as contrasted with that by purgatives, that Dr. Armstrong scarcely alludes to the latter as a remedy, although he does not seem to have had any more decided objection to them, than their uncertainty and inferiority. Whenever he thinks of comparing the effects of opium with those of any other remedy, it is only with blood-letting.—He tells us, he has witnessed some cases of inflammation of the bowels, where full doses of opium finally effected the cure, after bleeding and purging had completely disappointed his expectations. “ So great indeed,” he says, “ is my confidence in full doses of opium in peritoneal enteritis, that if compelled to say, supposing myself the subject of the disorder, whether I would exclusively rely upon them solely, or upon blood-letting solely,

I should certainly fix upon the former ; at the same time I should like to have the simultaneous influence of both remedies, being convinced they are by far more serviceable combinedly, than separately employed.”*

This was supposed to be a somewhat extravagant encomium on opium by Dr. Armstrong at the time. Since the death of that celebrated and excellent physician, Drs. Graves and Stokes of Dublin have directed our attention to cases not of very unfrequent occurrence, in which there is so much debility connected with inflammation, that one cannot look for advantage from depletion in any shape ; such are the cases occurring after tapping in exhausted dropsical habits, and of low puerperal peritonitis.—These physicians have also given some remarkable ones of peritoneal inflammation from perforation of the intestine, in which

* Dr. Armstrong first invariably bleeds to approaching syncope, whatever may be the quantity necessary to produce this effect. As soon as the patient revives, three grains at least of good opium, in the form of a soft pill, are given, and quietude is strictly enjoined, so that if possible sleep may be obtained. In some irritable habits less of the solid, and some fluid opium are prescribed, that the anodyne and sedative effects may be more quickly produced. Its effects, he tells us, thus administered, are to prevent a subsequent increase in the force or frequency of the heart's action, and a return of the abdominal pain, while it induces a tendency to quiet sleep, and a copious perspiration over the whole surface. In many instances this simple procedure will remove the inflammation at once, nothing being necessary, when the patient awakes, but spare diet, absolute rest and quietness, with an occasional mild laxative. He always visits the patients in three or four hours after giving the opium, and if there be pain on pressure in any part of the abdomen, with a hot skin, and quick jerky pulse, he bleeds again to complete relaxation, and repeats two grains of opium with four or five of calomel, in the form of a pill, as the faintness disappears. A third venesection is rarely requisite, but if, after the expiration of five or six hours, pain and fever still exist, it should be performed once more, and followed by the administration of a grain of opium, and two or three of calomel immediately, and half a grain of opium and two of calomel every four hours, until sleep and general perspiration are induced.

It is evident from the cases published by Drs. Graves and Stokes, and from the one above detailed, that the opium may be safely employed with much more freedom than Dr. Armstrong was accustomed to recommend. There is one precaution which it may perhaps be necessary to offer : I have two or three times found difficulty of passing water succeed the opium treatment, where it was perfectly successful, and in one instance, after a profound sleep, the patient was awakened by uneasiness from distended bladder, and could not evacuate it at all ; the catheter was introduced, and gave instant relief. When calomel is conjoined with opium, Dr. Armstrong very properly reminds us, that as the specific effects of mercury are most easily produced after copious abstractions of blood, we should use a proportionate care in its exhibition.

not only was bleeding altogether forbidden, but the operation of a purgative would have been certain death. These were in a worse alternative than that which Dr. Armstrong supposes for himself; there was no choice left them but opium, and it in every instance surpassed all the anticipations he could have formed of its unaided virtue.*

Perhaps no little creature was ever restored to life and health again, after having approached the verge of existence so closely, as a child, who was under my care about two years since—the following are the notes taken of the case.

CASE II.—A delicate girl, aged ten years, who had only been six or eight months recovered from a tedious and most alarming attack of chronic mesenteric disease, came in sick from the garden after having gorged herself with every kind of fruit. She took some tea and went to bed, but was awoke out of her sleep in two hours with violent pain in the stomach and side and disposition to vomit. She got warm water and discharged quantities of gooseberries and currants off her stomach, which had become quite tender to the touch. As the pain continued she was afterwards put into a hot bath and got castor oil and laudanum, after which she became easier and went to sleep. I saw her at twelve o'clock on that night. She was then resting very quietly—her skin cool and her pulse ninety. As very judicious remedies had been already employed, I thought it would be wrong to awaken her, although her countenance looked badly. She slept pretty well, but awakening, as I was informed, at five or six o'clock, got more castor oil, which had not operated at nine, when I came to see her. She was now alarmingly altered—her countenance was sunk and anxious, her pulse feeble at 130, the abdomen swelled and exquisitely tender, especially at the left side midway between the short rib and crista of the ilium. and the stomach inclined to vomit every thing. There was no spinal tenderness. She got two pills of aloes and extract of henbane, with five grains of calomel, and the former were directed to be repeated every two hours; an enema was also given, but it brought away nothing, and sixteen leeches were afterwards applied as near as possible to the spot where the tenderness of abdomen was most acute. I was afraid to bleed generally in consequence of the great feebleness of pulse, although the inflammation was evidently of a most

intense character. The sickness of stomach and vomiting came on always after drinking, and there was great restlessness and tossing in the bed. As the leeches continued to draw blood the weakness of pulse and restlessness seemed to encrease, the forehead became cold and clammy, the eyes looked sunk and the face drawn. I removed the leeches instantly, (they had been on about twenty minutes or perhaps half an hour) and closed the wounds with starch—meantime I gave her a teaspoonful or two of wine at intervals. The restlessness and sinking, however, still continued—she threw herself from side to side frequently, and could not be got to keep her hands quiet; the pulse was 150 and very feeble, and the vomiting still recurred as soon as a few teaspoonfuls of wine were taken. Altogether she presented just the appearance and manner of a woman dying of hemorrhage.—She did not complain much of the pain, although the tension and exquisite soreness of abdomen continued. I now gave her teaspoonfuls of burnt brandy with ten drops of laudanum instead of the wine, and as I was excessively alarmed about her, requested the assistance of another medical gentleman. The stimulants, although they did not remain long on the stomach, seemed to be of some service, as she rallied a little before the physician who was sent for arrived. The restlessness was less and the stomach quieter. A simple enema was now given and the brandy continued. In about an hour another enema was given with some turpentine. Both the injections came away together and brought off a quantity of currants and gooseberries, but no fæces. During the night the burnt brandy a little diluted, and sometimes wine, was given. An assafætida enema was administered twice but passed away after a considerable time without bringing off anything, and she got a little chicken broth. The restlessness and vomiting, however, still came on occasionally, the abdomen was rather more distended and exquisitely painful to the touch, so that she always cried when we approached to examine it—the bowels were not moved, there was some hiccup now and then, and the pulse was feeble at 160. She did not, however, complain of pain nor appear altogether so sunk as on the evening before, although she had got no sleep. This was the second morning since the commencement of the inflammation. I had been anxious from the first to give large opiates in this case, those I administered having produced no sensible effect. The small doses given could, in fact, be of no avail, as she usually vomited every ten or fifteen minutes or oftener, precluding the pos-

sibility of any accumulated effect from them. It was, therefore, necessary to give a large dose at once, if any decided benefit was to be attained by the remedy at all. This, however, was objected to by the Physician in consultation. The bowels, which were tense and somewhat tympanic, had not been moved since the inflammation commenced, and it was feared that a large opiate might *lock them up* permanently. It was considered preferable to give some aloes with croton oil in pills, on the supposition that any chance there was of recovery depended on getting the bowels freed. I suggested the danger that appeared in my view of the case in giving any purgative at all, and especially castor oil, while, if the opiate produced any effect, it would be most likely that of relieving the inflammation which would be a step towards recovery, whether the bowels were moved or not. As, however, the gentleman with whom I consulted, was a man of great experience and ability, as he was decided about the correctness of his opinion, and as I had then no experience of the effects of large opiates in a case precisely similar, I consented to his proposal, and two pills containing a few grains of aloes and quarter of a drop of croton oil were given to the girl. Very soon after she became much worse and towards noon the sickness of stomach, hiccup and prostration of strength became frightful. The pulse was scarcely perceptible at 160 or 170—the face blanched and covered with cold perspiration, the eyes sunk like a person moribund. The poor little sufferer flung herself from side to side, calling for breath, and continually throwing her clammy hands about restlessly. This state alternated with the vomiting, which, severe as it was, appeared the less distressing of the two to those who stood by. I had been feeding her with brandy and wine by teaspoonfuls every four or five minutes for the last hour or two. She took, I think, two or three ounces of white wine, and perhaps half a glass of brandy burnt within that time, with no perceptible effect. Convinced that she must now die unless opium gave relief, I measured twenty drops of laudanum into a little brandy and gave it to her. It appeared, however, to have no effect, for the feeling of sinking continued, and I once or twice heard the little creature utter in a faint voice, “I’ll die Doctor—I cannot live till morning.” She also told her father that she was going from him, and a scene took place that it is needless here to describe, but which could never be forgotten by any one who witnessed it. I just waited until half an hour had elapsed before repeating the sedative, and then gave a solid grain

of opium in preference to the laudanum—a large dose as a repetition to a child only ten years old. When another quarter of an hour had elapsed I watched with intense anxiety, and, I confess, with profound despondency, for the effects; but the tossing and restlessness and vomiting went on, and I expected every now and then to see her rise up, fling herself back upon the pillow and expire. Ten minutes more passed and there was a languor in her movements and less distress in her breathing, and when the half hour was gone by she was lying still and inanimate. I hoped the opium was influencing the nervous system. I stood looking silently and intently at her unusual quietness on the pillow, and while wondering whether it was to be attributed to the antagonizing power of this extraordinary medicine, my little patient was asleep. Physicians have wearying lives; they have more anxiety, more responsibility, than any other professional men, and they have continually to witness pain which they have no power of allaying, and scenes of misery which they cannot mitigate—but they enjoy some bright moments which go far to make them amends and which fall to their lot only.—This, to me, was one of them—for I saw that my patient was recovered.

She did not move until eight o'clock the next day, and she then awoke with a smile; she had neither hiccup nor sickness of stomach throughout the night and complained now of no pain. The tenderness and tension of abdomen however was as great ever. She took a little tea with a liking. Having witnessed the ill effect of the purgative the day before, we determined to leave the relief of the bowels to nature, and we were contented with applying a bread and water poultice to the abdomen. She passed the day quietly, took liquid nourishment—tea, coffee, or barley water, and slept frequently. In the evening she got a simple enema with five grains of aloes in it, and with this we left her for the night. At about twelve o'clock her bowels were moved, not very loosely as after a physic, but in considerable quantity, and the evacuation was of as natural and healthy a character as it could possibly be. She then got another grain of opium and went off to sleep. In the morning her bowels were moved twice naturally and freely, and the soreness of abdomen, though still extreme, was circumscribed to a patch between the left ileum and umbilicus, the very centre of which was excessively tender and had some feeling of fluctuation. This spot in the

centre was the point where the inflammation commenced, and was more tender than any other all through. The pulse was now 80, the skin natural—the countenance cheerful and the appetite returning. The opiate at night was continued. On the fifth or sixth day of her illness, when she appeared to be going on very well, an eruption appeared all over her person, exactly resembling mercurial erythema, or the rash which sometimes appears after recovering from cholera, at which I was excessively alarmed ; it went off, however, in two or three days without any ill consequence. The pulse afterwards quickened again ; she became a little feverish, and as the abdomen of the left side still continued tender, and we were apprehensive that an abscess might form, a blister was applied. The soreness was considerably less after the healing of the blister, and gradually abated. Eventually she became quite well, and is now a healthy girl.

I have been favoured by my brother with a report of a case scarcely less extraordinary than that just detailed. It tells so strongly for the influence of opium in one of those circumstances of disease to which Drs. Graves and Stokes have pointed it out as so particularly applicable, that I shall venture to quote it.

CASE III.—“ In the early part of this year I was called to see a boy, five years old, in the last stage of typhus fever. The illness had been very much protracted, and had produced great emaciation and debility. I found him lying partly on his back, and partly on his side in the bed, moaning, speechless, and insensible ; the wasting was excessive, and the pulse 140 and feeble, indeed almost imperceptible ; his features were quite drawn and pale ; his nose, cheeks, and forehead cold ; and on examining the feet and hands, they were found equally so ; his head had fallen aside on the pillow ; his breath was drawn by gasps, and at intervals, some seconds apart, like one expiring. When some drink was put into his mouth with a spoon, it brought on distressing cough from the difficulty of swallowing. On examining his abdomen, which was tumid, hot, and excessively tender all over, he moaned loudly. The only treatment he had been under for some days, was, the use of occasional doses of castor oil which had not operated ; his bowels had not been moved for three or four days.

“ If there had been more strength, bleeding would have been indicated by the state of the belly, and considering the state of the bowels, confined for three or four days, and now

tumid, hot, and tender, I believe, very many practitioners would have been tempted to employ at the least some mild purgatives. I recollected, however, those interesting cases by Drs. Graves and Stokes on the use of opium in cases of perforated intestine, and as ulceration of the mucous membrane, perforation, and peritonitis are not uncommon terminations of typhus, and this little fellow's symptoms seemed in a degree to indicate some such lesions, I ordered him a grain of opium with four of extract of henbane, more as a forlorn hope than with any expectation of its averting the impending dissolution. To my great surprise, he spent the whole night in a profound and quiet sleep. He took scarcely any drink, and the little he did take was given him with difficulty by a spoon ; but the tenderness of abdomen and moaning were much less ; the pulse slower and more steady ; and he shewed some signs of returning sensibility. The opium and henbane were repeated, and a dose of castor oil was given early on the following morning. When I saw him in the course of the day, the improvement was so great, that he could take a cup in his hand to drink, and from this time forward his recovery was certain. The opium was still repeated regularly for a few nights, and when the abdominal tenderness diminished, his bowels were easily moved by castor oil, of which he got a moderate dose daily. As the secretions were found to be much deranged, he was ordered small doses of calomel twice a day for a week or more, at the end of which period he was quite convalescent."

My brother's suspicions of perforation having occurred in this instance, would appear somewhat probable from the fact, that it may and does occur without the violent or sudden pain which has been usually stated as one of the principal symptoms. It would appear from an extraordinary case which lately came under my own observation, that in states of great attenuation and exhaustion of the system, perforation of the intestine with effusion into the abdominal cavity may take place, and continue until death without occasioning any pain or inflammation at all. As it is in some degree connected with our present subject, and for extent of intestinal disease, without the interruption of any of the living functions, is not a little singular, I cannot resist the temptation of offering it to the reader, at the hazard of prolonging this article a little unreasonably.

A fine boy, aged ten years, after a severe attack of measles,

during which the pulmonic symptoms were very severe, was affected with occasional pain in the left side of the chest, harassing cough and hectic fever ; which after some time were relieved by medical treatment. There remained only a great appearance of delicacy ; constant rapidity of pulse, and an incapability of much exertion : even in these respects he seemed to amend considerably during the summer, by change of air, and gentle exercise. The hectic fever and cough, however, recurred again, without any apparent cause, in the commencement of winter, and there was disorder and irregularity of bowels ; with other symptoms of mesenteric disease. He seemed to improve occasionally for short periods, and got on to the next winter. The symptoms of the affection of the chest were now entirely supplanted by the abdominal disease ; he had little cough ; no pain of chest ; but there was great rapidity of pulse with low evening fever, excessive languor and emaciation, with fulness and pain of belly, attended by a loose state of the bowels. When the fulness and tenderness were less than usual, the enlarged mesenteric glands were distinctly felt, especially in the right iliac region. The swelling of these glands progressively increased at the side mentioned ; where it was attended with so much pain and tenderness, that leeches were applied. Dr. Marsh, with whom I had consulted on the case at an earlier period, was again advised of the state of our little patient, and seemed to consider that an amount of tubercular disease had taken place, for the relief of which there was little hope. Besides ; the extreme weakness and the increasing irritability of stomach which existed, precluded any treatment beyond palliation of the symptoms. He had now generally from three to five free evacuations in the day, of a light yellow colour, and little consistence, and complained occasionally of pain in the belly, which was sometimes full, and at others very little so ; the changes were so frequent in this respect, that they were attributed to the absence or presence of flatulence ; but on examining the boy carefully, one morning, I found evident fluctuation. My relative, Doctor Geary, who saw him about the same time, agreed with me, that there was water in the cavity of the abdomen, and we concluded that the case was terminating in a very common mode, by ascites.—In a few days after, on examining the abdomen again, there was little swelling and no fluctuation, and later yet they were once more perceptible. I at last concluded, that the fulness

must be the result of mere distention of the intestines with fluid, and that the changes were attributable to my accidentally making the examinations immediately before, or after free evacuations. As the abdomen never afterwards swelled to any greater amount, I attached little importance to the feeling of fluctuation, and did not repeat my examinations with much accuracy; the tender knots or lumps had latterly altogether subsided, and the abdominal pains were seldom complained of; the looseness of bowels, however, was on the increase; the appetite was fickle and failing; the debility excessive, and there was low fever in the evenings. The little fellow was at this time removed to the country, and I did not see him for some days; he then appeared to me amazingly altered; his countenance had grown sharp and hollow; his voice weak; the motions of his lips tremulous; his pulse was rapid and thread-like; he was unable to sit up or to move about, though dressed daily and taken into the open air, and he was emaciated to the last degree. It was painful to see his naked person, it looked like an anatomy, over which the skin had been drawn. His parents told me, although the motions had become yet thinner than before, that they were unaccompanied by pain; that he had no perspiration; ate a little meat, or a few oysters, every day, and drank some porter; but he was notwithstanding declining rapidly. In a day or two after, as his mother was raising him from the bed to her lap, his countenance altered suddenly, his voice faltered, and he appeared to be fainting; they got him some wine and water, which he attempted to drink when held to his lips, but was unable; he laid his head on his mother's shoulder, gasped like a dying bird, and expired.

I have been particular in describing minutely the mode of the child's death in this very interesting case, as a proof of his having sunk from mere exhaustion. The examination of the body took place on the third day after. On laying open the abdomen, a pint or more of yellow fluid, very much resembling what he passed by the bowels, for days before, was found in its cavity. On raising a fold of one of the small intestines, a hole sufficient to admit the finger, with curled or thickened edges of a yellow colour, presented itself; it had somewhat the appearance of an opening made with a red hot iron—a look as if it had been burned out, and the same description of yellow fluid was flowing from it, as we found in the general cavity of the abdo-

men ; within two or three inches of this, was another perforation nearly as large, and to our astonishment, on tracing the canal upwards, we discovered perforations through its whole extent, from the termination of the ileum, to within a foot of the duodenum, which last as well as the large intestines were free from ulceration. *There were seven-and-twenty holes of various dimensions from some that admitted a quill, to others, more numerous, that a finger or thumb would pass through.* They were quite open ; the yellow fluid described flowing from them, when raised up ; and occasionally, when much handled, more consistent fæces, but of a similar colour. My brother, who assisted me in the examination, agreed with me in the conclusion, that there was no observable difference between the fluids found external to the intestines, and that which flowed afterwards from them, by the perforations. These last had by no means the appearance of having been recently formed ; they were for the most part too large and open to suppose so for a moment, and they were evidently not all of the same age ; some having minute openings with thin, flocculent edges, others larger ones and more ragged, and many of the largest with thickened margins, as if of old standing. Independent of these appearances, the vast number of them was much against the supposition of their having occurred suddenly, as it would be rather too much to believe, that twenty-seven ulcers perforated the intestinal canal in a day or a night, without occasioning the slightest symptom of such an event, during life. On examining the intestine on the inner side, the ulcers were found to be of much greater extent than the perforations would lead one to imagine ; the coats appeared to be gradually eaten through, in a less degree at the outer area of the ulcer, and greatest towards its centre, where they at length gave way ; there were many ulcers besides, which had not yet perforated the gut, although in most instances they seemed to have destroyed all but the peritoneal covering. In one only of the perforations had nature made any effort to accomplish a cure ; it was a large one at the lower end of the ilium, and the transit of faecal matter was prevented by effusion of lymph and agglutination of its edges to the adjoining fold of intestine ; the adhesion was very slight and readily broken through.

The mesenteric glands were considerably enlarged, some had supplicated in the right iliac region, and were filled with scrofu-

lous-looking matter. Two large abscesses filled with matter were also found near the root of the left lung ; through the substance of which minute tubercles were scattered ; the right lung was healthy ; the brain was not examined.

How long the perforations had existed previous to death in this interesting case, and whether the liquid contents of the intestines passed and re-passed through the ulcerated holes continually, are questions which must be solved by future observation. It is sufficient for us at present to consider the extent to which, in certain states of the system, a disorganizing complaint of this kind may go forward without evincing any marked symptoms by which it could be detected. We may at least deduce this inference from it, that in enteric inflammations occurring in exhausted habits, if preceded by diarrhœa or other marks of affection of the mucous membrane, perforation may take place without much or sudden pain. It is, therefore, necessary for us to be particularly vigilant and cautious in our treatment under such circumstances.

I shall offer only two cases more in illustration of the efficacy of opium in abdominal inflammation. They are, I think, as convincing as it is possible for any cases to be.

CASE IV.—A strong active man, aged thirty years, was seized with pains in the abdomen, chiefly about the situation of the umbilicus—the pain came on more violently at short intervals, but never ceased and was attended with extreme soreness of the abdomen, especially in the situation of the pain—the stomach was sick. He got castor oil and turpentine which was vomited up again. He afterwards got repeated doses of calomel and coloeynth with injections, but the former produced no effect and the latter came away as they were administered without any admixture of fæces. He was subsequently bled to the amount of thirty ounces or upwards, with some little relief to the pain and the purgatives were continued, but after some short time all the symptoms became worse. About ten o'clock at night the patient's friend became excessively alarmed at his encreasing illness, and desired a consultation, upon which occasion I was requested to see him.

I found him lying on his back, moaning faintly, and complaining of constant pain about the umbilicus, which, at intervals of a few minutes, encreased to a violent degree—he had frequent retching, and could retain nothing solid or fluid upon the sto-

maeh—his countenance was dejected—his pulse 100—his skin rather warmer than natural. He had had no movement of the bowels since the commencement of the attack. The centre of the abdomen was covered with a blister so that I could make no examination as to the degree of tenderness, but all the parts above, below and at the sides beyond the margin of the plaister, were excessively sore to the touch. The medical gentleman in attendance finding all his efforts to get the bowels moved were unavailing, was just preparing to give him some croton oil.

I represented to him, how very unlikely it was, that by any purgatives he could get an inflamed bowel to act—that inflamed muscles never do. That the chief object appeared to me to be to subdue the pain and inflammation, leaving the evacuation of the bowels entirely to nature, which would probably effect all that was desirable when not interfered with by an inflammatory condition of the parts. I proposed that the blister which had been on only about two hours should be removed, and that eighteen or twenty leeches should be applied about the umbilicus where the pain was most acute—that warm fomentations should be afterwards applied both to allay the suffering and encourage bleeding from the leech-bites, and that two grains of opium with two of calomel should be given immediately, with one grain of each every hour afterwards until the patient fell asleep. On the medical attendant expressing some alarm at the quantity of opium which might be given in this way, and the bowels so obstinately constipated, I observed, that if sleep could be procured, the man would probably awaken freed from all pain, and have his bowels moved without the necessity of a purgative.

The leeches were applied and the bites bled freely with the fomentations. The opium remained on the stomach, and soon after the patient took the third dose the pain evidently abated.—In less than half an hour he became easy, had a heavy look, and before the hour came round for the next dose was in a sound sleep. At seven o'clock in the morning, after six or seven hours uninterrupted sleep, he awoke freed from pain or sickness, and on being assisted to the night chair, had a free, easy, and copious evacuation. He had one or two more motions in the course of the morning and required no further treatment.

In two days after, this patient had a slight recurrence of the pain from imprudently exposing himself to cold by going into the open air. His medical attendant ordered him castor oil—

the pain, however, continued, and was hourly on the encrease until he recurred to the opium and calomel, of each of which he gave two grains with the same result as before.

Had the ordinary purgative treatment been adopted in this case, I feel convinced it would have gone on from bad to worse, and that at the end of two or three days the bowels still unmoved would have become tympanitic or gangrenous.

CASE V.—A woman, aged 30 years, and about six months pregnant, was seized with violent pain between the short ribs and ileum at the right side of the abdomen, extending to the umbilicus. The pain was excessively severe, and came on in paroxysms—the bowels were confined. She took medicine—got an enema and was stuped without the least relief. After suffering in this way for three days, a medical man was sent for, who immediately bled her and ordered her purgative medicine. The latter was thrown up, but some castor oil which she took afterwards remained on her stomach and purged her freely. There was, however, little or no relief, and after the lapse of two days more of intense suffering, I was sent for to visit her. On my arrival I found her lying on her back in the bed with panting respiration, at about 40 in the minute, and constant moaning. She could not bear to be stirred in the slightest degree, or to move her lower limbs, in consequence of the soreness of abdomen, nor had her bed been made for some days. The slightest pressure on any part of the abdomen—the least attempt at turning on her side, or even the motion of her lower limbs, occasioned excruciating pain, and obliged her to scream out. The pain became more violent by paroxysms, though never subsiding, and the tenderness was more acute in the right side where the affection commenced than at any other part of the abdomen.—The pulse was about 120—the tongue coated, and she complained of thirst, but there was no considerable sickness of stomach, although she occasionally rejected her medicine.

This case presented altogether so unfavourable an appearance, that I entertained very little hope of doing any good. She had been totally neglected at first. The bleeding was not repeated as it ought to have been, and the castor oil, though operating freely, gave no relief. A general bleeding appeared to me to be now a dangerous remedy, as the vital powers were worn down by the long protracted suffering, and considerable organic mis-

chief had already taken place. A grain and a half of opium with two grains of calomel were directed to be given every half hour for two doses, then a grain of the former with two of the latter every hour until she should either feel relief from the pain or fall asleep, and eighteen leeches were applied to the right side, followed by fomentations, and finally a gruel poultice. After taking nine pills in the course of the night, she got ease and fell into a quiet doze towards morning. This lasted for several hours, but towards evening there was a recurrence of the pain, and a pill, containing a grain of pure opium, was given every hour until relief was again experienced, which did not occur until next morning, when six or eight more of them had been taken. I now saw her again, and found appearances greatly altered. The quick respiration had altogether subsided—the pulse was slower—the pain nearly gone—the tenderness of abdomen, except at the right side, greatly abated, and she could turn a little in the bed. A purgative enema with about an ounce of turpentine and two quarts of oatmeal tea was now administered; pills, containing equal parts of aloes and extract of henbane, were directed to be given every second hour, and, if the pain recurred, a repetition of the opium pills. The bowels were abundantly moved after the lapse of a few hours, and there being a slight recurrence of pain afterwards the opium pills were resumed, but only at intervals of three or four hours, after which they were wholly discontinued.

I heard nothing of this patient until four or five days afterwards, when she drove on a car eight miles to see me. She was in every respect recovered, except in the tenderness of the right side, which still continued acute in the original site for about the breadth of the palm of the hand. This was evidently attributable to some organic mischief which was done to this part before any effective remedy had been resorted to for the inflammation. Repeated leeching—fomenting, and small doses of calomel and opium were ordered for her, and she eventually became quite well.

It must not be imagined, from all I have stated, that I believe purgatives to be at all times inadvisable in enteritis. There are perhaps occasions in which they are of use even in the early stage, but it is difficult to offer indications by which we shall recognize these occasions in practice. In the advance of the disease, when its force is considerably broken, and the bowels may be

supposed capable of acting without increasing or renewing the inflammation, there must be an obvious advantage in getting rid of the contents of the bowels, and this may perhaps be then generally effected with safety, by means of mild purgatives, combined with henbane. If, however, there was no injurious distention present, and the inflammation was progressively declining, my disposition would be, to await a more perfect amendment before I would give even these. The only evils I should at all apprehend in these instances from confinement of the bowels are, irritation and uneasiness from restraining the natural actions and secretion, after once the decline of inflammation admits of their taking place ; or at a later period, from actual distention by the intestinal contents.* These are certainly evils to be held in view and guarded against, though I believe by no means of that vital importance with which they have been heretofore commonly invested. Without pretending to have satisfactorily solved the problem which I have yet ventured to discuss at such length, I shall merely recapitulate a few of the principal facts, as far as they appear to be such, and leave the inferences to the profession. General experience testifies, that the strongest purgatives will not operate in the early stages of inflamed bowels unless large depletion by the lancet has been premised, that is, unless the violence of the inflammation has been in some measure subdued : while on the other hand, as soon as this has been accomplished, they commonly occur spontaneously, or with the assistance of the mildest purgatives.

Notwithstanding the free operation of purgatives at an early stage of enteritis, the inflammation may proceed to a fatal termination, unless arrested by other remedies.

A purgative has been known to occasion inflammation of bowels, and when inflammation has been subdued by other remedies, it has brought on a recurrence of it.

Inflammation of the bowels may be perfectly subdued without any evacuation at all.

The bowels may even sometimes continue in a confined state for three or four days after the inflammation has subsided, without occasioning injurious distention.

* As mere distention of the intestines may occasion or keep up tenderness, it may happen after all inflammation and pain have been reduced, that the tenderness on pressure should continue. If there was great debility in such circumstances it might be a fatal mistake to withhold support or stimulants on the presumption that inflammation was still continuing. One's judgment should always be formed from the aggregate of symptoms.

PROBLEM II.

BY WILLIAM GRIFFIN, M.D.

HOW ARE NERVOUS AFFECTIONS DISTINGUISHABLE FROM INFLAMMATORY.

IN treating of inflammation of the bowels and peritoneum, Dr. Abererombie states as conclusions from facts, "that extensive and highly dangerous inflammation may exist in the intestinal canal without obstruction of the bowels, and it may go on to a fatal termination while these are in a natural state or easily regulated by mild medicines through the whole course of the disease."

"That extensive and fatal inflammation may be going on with every variety in the pulse; it may be frequent and small, it may be frequent and full, or it may be little above the natural standard through the whole course of the disease."

"That extensive inflammation may go on without vomiting and without constant pain; the pain often occurring in paroxysms, and leaving long intervals of comparative ease."

"Keeping in view these sources of uncertainty," he says, "our chief reliance for the diagnosis of this important class of diseases, must be on the tenderness of the abdomen. This symptom should always be watched with the most anxious care, *whatever may be the state of the bowels or of the pulse, or the actual complaint of pain*, and though the tenderness itself should be limited to a defined space of no great extent."

Whatever the experience of the profession may have determined with respect to the greater part of these conclusions before the appearance of Dr. Abererombie's work, I believe the importance which should be attached to the presence or absence of abdominal tenderness in cases of suspected inflammation has been always in accordance with the opinions of that distinguished physician; but Dr. Abererombie was very well aware, when he gave such just weight to this symptom as a means of diagnosis, that in itself it was far from conclusive, and derived its value chiefly from the existence of many other corroborating signs. He states indeed distinctly in another part

of his work, " that pain increased upon pressure does not appear to be a certain mark of inflammation in the bowels, for it occurred in Case XXIV., (related by him,) in which there was no inflammation ; and in several other cases it was met with probably before inflammation had commenced. From various observations (he states) he is satisfied, that intestine which has become rapidly distended is painful upon pressure ; it is, however, a kind of pain which, *by attention, can generally* be distinguished from the acute tenderness of peritonitis."

This point of diagnosis applies altogether to the discrimination of cases of ileus from those of inflammation, and even as such, is, it would appear, often of doubtful value ; but in what light must we view it in reference to those frequent neuralgic affections, which, whether hysterical or the results of irritation of the spinal cord, are often established suddenly, with little preparatory disorder, and with no distention of the intestinal canal to account for the acute tenderness on pressure ? No medical man is now ignorant of the fact, that the contents of both the thorax and abdomen as well as their parietes are subject to attacks of violent pain of a nervous or spasmodic character, yet with acute soreness to the touch, as in pure inflammations. These are so widely separated from the latter in their nature, and require such a very different, I might almost say opposite mode of treatment, that a correct diagnosis becomes a matter of still greater interest than in cases of ileus. There are few instances of ileus in which one effective bleeding might not be of service, while there are few of hysterical pseudo-peritonitis, or enteritis, or of similar affections arising from irritation of the cord, which might not be made worse, or indefinitely protracted by it—yet we find in our best elementary works on this subject a most perplexing indefiniteness, a diffuseness of description, and a labouring at discrimination wholly unworthy of the present improved state of medical science, and unnecessary if any real or essential difference of character could be pointed out. It is indeed, because there is an essential agreement in all material points, that our attention is directed to the attitude, the expression of countenance, the manner of complaining, and even the temper of patients, although there must be sometimes very considerable differences as to all these in individuals similarly affected, and in any case, no slight degree of experience is required to form a proper estimate of their value. We are told

that one affection is frequent, the other rare in comparison ; that females are more liable to it than the other sex, and those of sedentary habits in towns, than those leading active lives in the country ; all of which might be very useful information, so far as it tended to corroborate features of a more marked expression, but is essentially loose and vague, if considered by itself.—Again we are reminded of one or two truisms, that if a disease has lasted long and done no mischief, or if it has been aggravated by an antiphlogistic plan of treatment, its character is not inflammatory. We may thus arrive at a correct diagnosis about the time the complaint ought to be cured and forgotten. But the climax of our difficulties on this subject is displayed in the admissions of experienced physicians, that their practice when in doubt, is to run the risk of erring on the safe side, and treat the disease as inflammatory ;—perhaps a judicious plan enough under the circumstances, but certainly not so consolatory to the patient, nor as I have elsewhere remarked,* very creditable to medical science. It may be questioned too, whether a rule of the kind with young physicians would lead only to errors on the safe side. I have seen one valuable life lost, and others endangered by such practice, and sometimes the antiphlogistic treatment vigorously adopted, not indeed where doubts existed in the mind of the practitioner, but where the cases were altogether mistaken.

I am far from assuming to myself a perfect freedom from perplexity in all possible cases simulating inflammations of internal organs which may come before me, but with the facts I have stated impressed on my mind, I cannot but feel that the experienced physician is usually too well satisfied with that indefinable power of recognizing or identifying diseases almost unconsciously acquired in the course of long practice. In reflecting on the melancholy steps by which it is attained, on the sad, though perhaps excusable errors committed, he consoles himself with the conviction, that such experience only could gift him with a knowledge which neither books nor lectures had taught him ; when he should rather feel, that the true reparation to his conscience, the real duty he had to perform in acquittance, was to prevent the occurrence of such mistakes with others, by analyzing the characters of those perplexing diseases, and endeavoring

* See Treatise on Functional Affections of the Spinal Cord.

vouring to trace the sources from which his late discrimination was derived. It is not enough for a practitioner in a difficult case to be assured he is himself capable of determining its nature; he should consider whether his knowledge or ability admits of being communicated to others, and when he believes it is, but not until then, he may also believe that he has acquired information of infinitely greater value to the public, than any thing it could have suffered from his early mistakes.

I have been led to indulge in these observations in consequence of the apparent inattention with which a suggestion with respect to the diagnosis of neuralgic affections, proposed by me some years since, was received by the profession: I then stated, that in any doubtful cases I believed if tenderness on pressure *at the portion of the spinal column corresponding with the disturbed organ* existed, it might be considered decidedly neuralgic, but if no such symptom was found, it was probably inflammatory.—The suggestion was then offered rather as a result of individual observation, which I was anxious should be tested by the universal experience of the profession, than with a view of claiming attention for an incontrovertible fact; but I have since so repeatedly derived ready assistance in simulated inflammation by assuming it as such, and have had the diagnosis so invariably borne out by the result, that I do not now hesitate to assert, it is almost the only single symptom upon which a young practitioner can rely without danger.

Without entering into the question of the nature of spinal irritation, which I have discussed at large in another place, I may be permitted to claim the reader's attention to two points connected with it, which must be regarded as physiological facts.—That the spinal cord, as the experiments of Le Gallois have shown, is composed of portions independent of one another in their powers and functions, being centres from which the nervous actions of corresponding parts of the body emanate, and to which they tend; and, secondly, in conformance with the well known law, by which the pain and tenderness arising from disorder at the origin or trunk of a nerve, is referred to, or felt at its extremity, that affections of the spinal cord are not usually recognizable by pain at the part diseased, but at the terminations of the nerves in distant organs arising from it. From these facts there is one undeniable inference in determining the diagnosis of cases resembling inflammation,—that wherever spinal tenderness

exists, we must at all events set down pain and tenderness (the two most important symptoms in assisting us to detect internal inflammation) as wholly valueless; inasmuch as, whether there be inflammation or not, these are not peculiarly the results of it, but may arise also from the tender state of the cord. I might perhaps go much farther, and assume, that where spinal tenderness exists, there also exists a state of the system scarcely compatible with acute inflammation.

If, then, pain and soreness on pressure, those supposed characteristics of inflammation, are of all others the most equivocal symptoms, belong as certainly to irritative as to inflammatory affections, and almost necessarily exist if there be acute spinal tenderness; what is to be our guide in deciding the diagnosis when called to a person suffering with violent pain of side, feverishness, difficulty of respiration, and soreness of the intercostal muscles; or with violent pain in the abdomen, accompanied by exquisite tenderness to the touch, and perhaps constipation and vomiting—*if we do not examine the spinal column?* We want to ascertain the simple fact in the first instance, of whether the pain is in the viscus supposed to be affected at all, or whether it be merely in the thoracic or abdominal parietes. And since the patient shrinks and complains on pressure in both cases, what, I ask again, is to be our guide, *if we do not examine the spinal column?* Are we to depend upon speculations on the attitude, expression of countenance, temper or manner of a patient, when we can at once lay a finger on the spine, and detect both the cause and nature of his complaint.

If in the former case we find acute tenderness of some of the dorsal vertebræ, or in the latter of some of the lumbar, I do not hesitate to say, the one is not pleuritis, nor the other enteritis, nor will either bear large depletion with impunity. The pain and tenderness are merely referred to the extremities of the spinal nerves, ramified through the intercostal or abdominal muscles, from the affection of the corresponding portion of the spinal column, and indicate nothing whatsoever of the state of the viscera internal to them, which usually excites so much alarm. If these are facts, and I believe few will deny them to be so on examining for themselves, is it not absolutely leading the young practitioner into those mistakes which we so much deprecate, to associate pain and tenderness on pressure so exclusively in his imagination with inflammatory disease? Is it

not strange too, that cases are every day published in our periodicals with dissertations on the difficulty of their diagnosis, without the slightest allusion to the spinal cord, the state of which, in every case of presumed inflammation, our present knowledge of physiology must shew us the necessity of ascertaining. One would suppose where the obscurity of the diagnosis is often so undeniable, that any adventitious light which could be brought to bear upon it would be sought for with avidity, but this, every day's experience assures us, is very far from the reality. As illustration is very generally more impressive than argument, I shall offer two or three cases to the consideration of the reader.

A young woman aged 25 years, was attacked with pain in the bowels at night after a feeling of chilliness. She took some essence of peppermint and went to bed, but the pain gradually increased, and at 2 o'clock in the morning she took twenty drops of laudanum. At 7 o'clock, the pain still continuing, she took castor oil with ten or fifteen more drops of laudanum by the directions of an apothecary, and soon after ten drops were repeated in a saline draught. I saw her at 1 o'clock, and found her writhing with pain, chiefly round the umbilicus and to the right side. It became more violent by fits like colic, though never entirely subsiding, and during the intervals of comparative relief she sometimes threw her arms about restlessly, sometimes lay as if insensible with the eyes turned up and the lids half open. Her complaints were low, scarcely audible, her respiration painful when deep; turning from side to side increased her pain, although when the paroxysm occurred she turned sometimes on her face. There was excessive tenderness of the abdomen; the least pressure making her scream.—She had been constantly vomiting for the last few hours. The castor oil had operated once scantily, her pulse was but little quickened, and there was no heat of skin.

Here was a case of constant pain in the abdomen chiefly about the umbilical region, liable to severe exacerbations, attended by exquisite tenderness on pressure, vomiting and constipation, and continuing for twenty-four hours. I am convinced that almost any young physician would have felt great difficulty, indeed almost an impossibility of determining from a consideration of the symptoms, that the complaint *was not inflammatory*, and I believe that the great majority of either young or experienced

ones would at once infer the existence of inflammation, and bleed. I say so from having witnessed it, and from having early in my own professional life always prescribed in such cases with timidity, as if I felt that all consideration of the symptoms led to little better than conjecture. I had now, however, new means of diagnosis in the state of the spinal column, on examining which my mind was set at rest. As soon as I pressed on the upper lumbar vertebra the girl started violently, caught my hand, and complained that I had hurt her dreadfully: the pressure, she said, had increased the abdominal pain; she had never had any hysterical attack in her life. I ordered the abdomen to be fomented freely, and gave her five grains of calomel with half a grain of opium, directing five grains of aloes and five of extract of henbane, to be taken every two hours. The calomel and the first dose of the aloetic pills remained on the stomach, but the succeeding doses were rejected; a purgative draught was also thrown off; she then got a purgative enema which was repeated in an hour, but both passed off without any appearance of fæces. The vomiting, pain, and tenderness of abdomen continued very severely throughout the evening. At night she got a turpentine enema, and was placed in a hot bath which gave some relief and procured a scanty evacuation. The pain, however, soon recurred, and she was ordered a grain of opium, to be repeated every hour until it should subside. The bowels were freely moved soon after taking the first dose, but she did not experience any considerable relief until she had taken the third, after which she fell into a sound sleep, and in the morning was in every respect improved. The pain had altogether subsided, and the exquisite tenderness was now felt in the right epigastric region only. She had threatenings of a return of the attack in the course of the day, but it was readily subdued by a repetition of the opiate. On the following morning there was scarcely any pain or tenderness, and if the complaint had been inflammatory, I would now have left the patient's bowels perfectly at rest. But believing it to be an affection of the spinal cord, arising from disorder of the digestive functions, I ordered another dose of castor oil, which operated freely, and was followed by no recurrence of the attack.

Mrs. —, aged 30 years, of a full habit, complained of pain in the right iliac region, which gradually increased until the

whole of the abdomen became sore to the touch ; on the third day she took castor oil, which operated freely, but gave no relief. I saw her on the fifth when she complained of being much worse, and directed her to get to bed, to have the bowels fomented, and to take pills of aloes and henbane every second hour until relief was obtained. In the morning I received a letter, stating, that she had been in agony all night, was much swelled, had not had her bowels moved for three days, and was now feverish and throwing every thing off her stomach. I became excessively alarmed, and regretted I had not bled her on the night before. On arriving at her residence I found her complaining of violent pain all over the abdomen, but most acutely in the right iliac region, and almost down to the pubes at that side. There was some fullness of the bowels, and the greatest tenderness on pressure. She could not bear the slightest touch in the iliac region, and one spot she described as exquisitely sore. It was on this account, perhaps, that she bore the weight of my hand laid flat, and pressed very gradually, better than pressure with the points of the fingers, as in the former case the pressure on the most painful spot was not so direct. Her pulse was ninety-six, her skin feverish, and tongue white ; she had nausea and occasional vomiting, and could not turn or move in the bed, without considerably increasing her sufferings. The only symptom that could lead me for a moment to doubt, that I had here a case of very serious inflammation to deal with, was the expression of the countenance, which did not betray the deep anxiety and distress I have usually seen in the inflammation of vital organs, although her own description of her suffering was sufficiently alarming.

The treatment of the case with decision and confidence now depended altogether on the examination of the spine. If I found a state of increased sensibility and excitement of that part of the cord which gave origin to the lumbar nerves, the pain and tenderness of abdomen would be accounted for, and I need not infer a more alarming cause for them. If, on the other hand, there was no spinal tenderness, the existence of acute inflammation was almost certain, and the most active treatment was demanded. On pressing the spinal column as soon as I reached the lumbar vertebræ, she started and screamed aloud. She also felt excessive pain on my touching the sacrum, or making pressure behind the trochanter, or at the front of the hip joint in the groin. I therefore unhesitatingly concluded, it was one of those

nervous affections simulating inflammation, in which the progress of the case fully bore me out. A few leeches were applied to the most painful part of the abdomen, and a grain of opium was directed every second or third hour until there was some abatement of pain; she was also directed as soon as the vomiting abated to resume the pills of aloes and henbane, and take a dose of castor oil. She got considerable relief, but passed a restless night. In the morning she took the aperient medicine, which operated freely, and now had pain occasionally only, and at long intervals; the vomiting had ceased, but the soreness of the abdomen was still exquisite in the right ileum and groin, and she screamed when I pressed on the vertebra or immediately behind the trochanter. Fomentations were now directed; belladonna plasters were afterwards laid over the painful parts, and the opium pills were given every fifth or sixth hour. She spent the day well, but the pain recurred in the night very severely, and was relieved by the anodynes. Next morning the bowels were again moved, but the soreness on pressure or on moving much in the bed still continued. The case went on in this way for several days, abating very gradually, but the soreness of the abdomen and of the hip joint was not perfectly removed until she was for some days driving out in her carriage.

I attended this lady afterwards for an attack of acute bronchitis, which was of a similar character in its advanced stages.—The cough, oppression, and expectoration, were evidently the result of nervous irritation, and required tonics rather than the continuance of any antiphlogistic treatment. She told me that in a former illness, which she called inflammation of the lungs, the doctor had bled and I believe blistered her more than once, that she was going on from bad to worse with incessant cough and debility, until at last, in defiance of all advice, she took a glass of pure wine, which cured her like a charm. I mention this, not that one can infer much from such loose statements, but because it gives some idea of the irritability of her nervous system.

Mrs. ———, of a delicate, nervous habit, after a natural labour, had internal hæmorrhage, with pallid countenance, cold clammy sweats, chilliness, and an almost imperceptible pulse at 156; the hæmorrhage ceased on the extraction of several clots, but the debility continued to an alarming degree for several hours. She had large doses of laudanum with dilute sulphuric

acid through the night, which seemed to relieve her much ; but in the morning the debility was still great, the pulse weak at 130, the features sunk, and the respiration much hurried. On the following evening she had a severe rigor, and was soon after attacked with pain and tenderness in the uterine region, excessive pain in the head, brow, and eyes, with sickness of stomach and vomiting ; there were thirst and heat of skin, and the pulse became rather hard at 140 ; the tenderness on pressing over the uterus was considerable, the pain constant, the lochia diminished. On examining the spine, there was found acute tenderness of the lumbar vertebræ, upon which it was assumed to be a case of nervous irritation and not inflammation. A dose of calomel with extract of henbane was given, fomentations were applied to the lower part of the abdomen, and she got diaphoretics at intervals. Under this simple treatment the symptoms declined, and on the succeeding night she felt quite relieved ; the lochia and secretion of milk becoming abundant, and the pulse soft at 125. After the lapse of some days she was up and well.

Mrs. M——, aged 26 years, had very severe flooding in her lying-in. On the fifth day after, she was seized with pains in the abdomen, attended by excessive soreness to the touch, and fever. She got castor oil on the sixth day, which operated twice without giving much relief ; in the evening she had a slight rigor and felt very weak ; the milk had left her from the first moment of the attack, and the lochia had nearly ceased. I saw her for the first time, on the night of the seventh day from her delivery, and found her lying on her back, with her knees up, complaining much, and incapable of the least motion, or turning to one side or the other without the greatest torture. The pain was constant, but was increased to an excessive degree at intervals ; coughing was very distressing to her, and the least pressure on any part of the abdomen made her scream ; slow, steady, cautious pressure with the flat of the hand was unbearable, but she could stretch down her limbs without increasing her sufferings ; her countenance, which was pale from the flooding, had an expression of distress, and the brows were knit as with pain ; the skin was warm, the tongue moist and white, and the pulse weak, at 112 ; there was excessive tenderness of all the lumbar vertebræ. The abdomen was fomented, and she got two grains of calomel and a grain of opium every third hour. After the second dose the pain abated, and she got a good deal of sleep, but in the morn-

ing the symptoms returned, and the pills were resumed with the same good effect as before. The tenderness of abdomen was now rather less, though the bowels appeared more full, not having been moved for the last forty-eight hours; she could turn from side to side with less suffering, the pulse still 112, and the skin warm and moist; she took castor oil, and afterwards pills of aloes and extract of henbane every second hour, which operated freely; she passed a tolerable night and seemed easier, but got the pain more violently than ever on the following morning, when it was relieved by 40 drops of laudanum; it continued in the intervals of these paroxysms, but not severely. In the evening the paroxysm recurred, and was again relieved by the laudanum. Notwithstanding these recurrences she felt herself on the whole much better, could turn from side to side with more freedom, and wished for nourishment; she complained of some headach. She was now directed to take three grains of sulphate of quinine three times a day, and to repeat the landanum if seized with the pain; I also allowed her to get a little chicken broth or beef tea. She passed a good night, and although the pain recurred at intervals on the following morning, it was much less severe; she threw up some aloetic and henbane pills which she had taken to free the bowels; the abdomen, though still painful on pressure, was much less tender; the soreness of the spine was nearly gone, and her milk was returning. As her bowels had not been moved for the last thirty-six hours she got some castor oil, and the quinine and broth were continued; she complained much of headach.

The pain recurred again violently in the night, soon after which the flooding returned for a short time, and then ceased; the pain was relieved by a grain of opium; felt much better through the day, and ate a little meat without permission; the soreness of abdomen almost gone, and none whatsoever of the spine; her headach was better; she continued her quinine and nourishment, and was well in a few days.*

* The above case at the first moment I was called to it, so perfectly answered the descriptions given of simple peritonitis, (see Abercrombie on the Viscera, p. 151,) and so truly resembled the cases of that disease which had fallen within my own experience, that I watched its progress with much anxiety. I was indeed somewhat distrustful of the diagnosis, for although the extreme tenderness of spine would fully account for the tenderness of abdomen, the great torture experienced on the

I might multiply cases of this nature to an extent that would be tiresome, without making the point much clearer. Perhaps it may be said, that an experienced eye would have detected some anomalous symptoms in all of these, which would have led to doubts of their inflammatory nature. I am not disposed to deny that much may be inferred in such attacks from the suddenness with which violent symptoms supervene, the absence of deep distress and anxiety of countenance, and above all from a freedom in the movements of the lower extremities, unusual in acute abdominal inflammations. There was indeed at least one of these discrepancies observable in a greater or lesser degree in each of the cases detailed ; but how are young practitioners to form a diagnosis on such grounds. People suffer similar degrees and kinds of pain with very dissimilar degrees of fortitude, and at all events any reasoning on such signs must be founded on comparisons, which, to be worth any thing, would imply an experience no young practitioner can be supposed to possess.

In all acute inflammations of vital organs, I believe that no spinal tenderness will be found, except where it existed previous to the supervention of the attack,* or where the spinal cord itself happens to be the seat of such inflammation. In all neuralgic affections on the contrary, tenderness of some portion of the spinal column, *usually that corresponding to the affected organ* may be detected, except in some rare cases in which it seems probable the ganglionic nerves alone are concerned. As these cases must still present a difficulty in their diagnosis, we

least motion from side to side, was a symptom more characteristic of true peritonitis than of a neuralgic affection ; the progress of the complaint, however, fully satisfied me of its nature. I believe there is no practical physician will assert that a case of acute peritonitis would have been arrested by the treatment adopted, which was in fact little else than palliative. It was only on the first day of my attendance that the opium, combined with calomel, was given with any regularity, five grains having been taken within twenty-four hours ; for although the pain recurred with equal violence subsequently, she had seldom occasion to take an opiate more than once or twice in the day. I may, however, point out one symptom in which the case differed from peritonitis—the ease and freedom with which she could stretch down her limbs. In spinal affections the least motion of the spine, especially turning or twisting motion, will sometimes increase the abdominal pain, but it scarcely ever occasions difficulty or uneasiness in the motions of the lower limbs.

* Some writers on spinal irritation state that they have found spinal tenderness with inflammation of liver.

must rest contented with those general characteristics, which, however vague or liable to lead us into error, are all we have to guide us, and all we have hitherto had to determine our opinion in that large class of neuralgic affections, for the detection of which I have here been offering a new, and I believe less doubtful sign.

The observations offered respecting the diagnosis of cases resembling acute inflammations, apply with almost equal truth to those resembling chronic diseases; I mean those pains affecting the chest, attended by cough and perhaps oppression, leading to apprehensions of phthisis; affecting the side below the false ribs and suggesting affections of the liver or bowels; or affecting the pubic region and simulating disease of the womb or ovaries. It is, however, wholly unnecessary to extend this paper to an unreasonable length on the subject of those nervous affections, the nature and diagnosis of which I have discussed so fully in another place. I need only remark, that deducing inferences from acute or chronic pain apparently affecting any viscus, without examining the state of the spinal cord, seems to me little less absurd than omitting to examine the state of the hip joint in those painful affections of the knee, the nature of which is not immediately obvious. In either case the disease may not exist at all in the part apparently affected, and the real source of the pain can only be ascertained with certainty by an examination of the origin of the nerves distributed to it, or of any organ with which the part affected may be supposed to sympathize.

PROBLEM III.

BY D. GRIFFIN, M.D.

UNDER WHAT CIRCUMSTANCES, AND TO WHAT EXTENT, IS BLEEDING PROPER IN DISEASES OF THE BRAIN?

DR. CHEYNE, in his work on apoplexy, thus writes—" I am desirous of removing every objection which can be opposed to blood-letting, which I am convinced is not only the most effectual remedy in apoplexy, but is much more effectual than all the others in use." In another part of the same work he speaks in the following terms—" Two pounds of blood ought to be removed as soon as possible after the attack, and if the first bleeding has not been of service, and the disease is unequivocally established, the chief question to be decided is the additional quantity of blood to be drawn. It ought to be known that from six to eight pounds of blood have been taken from a person by no means robust, before the disease, which ended favourably, began to yield. The first and second blood-letting ought to be large, and a third ought to follow the second as soon as it is ascertained that this has been ineffectual in stopping the disease." Again, he goes so far as to say—" I believe it is a good rule *to have every patient in apoplexy, who is not plainly dying, bled,*" and in another passage, relating to the same remedy, he is more absolute still. I quote from memory, but I think his expression is—" Should these means fail *the patient will stand in need of such attentions only as the dying require.*"

By far the greater number of our modern writers have in general terms strongly recommended the same remedy. Among the most strenuous advocates of it may be mentioned John Hunter, Cullen, Piteairn, Cooke and Abercrombie. It is not necessary to give extracts in support of the practice from each of these authors. I shall confine myself to the first and last named, to shew the extent to which the remedy was recommended. John Hunter says—" The only difference between apoplexy and hemiplegia is in degree, for they both arise from extravasations of blood." In these cases he thinks—" We ought to bleed very largely, especially from the temporal artery,

till the patient begins to shew signs of recovery, and to continue it till he might begin to become faintish."

Dr. Abercrombie says—"Our first great object is to take off the impulse of blood from the arteries of the head, by bleeding carried to such an extent as shall powerfully and decidedly affect the system, and by repeating it at short intervals as soon as these effects begin to subside. The first bleeding should probably be from the arm, but after this there seems to be an evident advantage in abstracting blood locally, either from the temporal artery or by cupping." He says afterwards—"In the extent of our evacuations, indeed, a due regard is certainly to be had to the age and constitution of the patient and the strength of the pulse; but I think we have sufficient ground for saying, that there are no symptoms which characterise a distinct class of apoplectic affections, requiring any important distinction in the treatment, or in other words, a class, *which, in their nature, do not admit of blood-letting.*"

In contradistinction to these recommendations we have the following observations on this mode of practice from other authors. To begin with the earlier modern writers, Dr. Fothergill says—"Bleeding in apoplexies is one of those operations which on several accounts requires the most dispassionate consideration. In no disease, perhaps, is the judgment of the prescriber of more consequence to the patient. If it is successful—if the patient recovers upon it—it is a fortunate event for both. If bleeding is performed when it ought not, either death ensues or an incurable hemiplegia." In another part, after assuming some circumstances in which it may happen to be useful, he says—"It is possible, likewise, that by a copious bleeding the animal strength may be so much reduced and the effort began so powerfully checked by the operation and the effects of the disease itself, that the patient expires soon afterwards or survives a few days and suffers a hemiplegia, *none of which might probably have happened had bleeding been omitted.* It becomes the operator, therefore, most carefully to attend to every circumstance of his patient's situation before he opens a vein *which may, perhaps, be decisive of his patient's fate.* Again he says—"It seldom happens that a physician arrives before this operation is performed, let the disease have originated from what cause it may, and though very few would probably recover if this operation had been omitted; yet comparing what has happened to those

who have been blooded and the few I have seen who have not, I am of opinion, that bleeding in apoplexy is for the most part injurious, and that we should probably render the most effectual aid by endeavouring in all cases to procure a plentiful discharge from the stomach and bowels, as by these revulsions, the head is perhaps much more effectually relieved from plenitude, and that without weakening or interrupting any other effort of nature to relieve herself than by venesection." Dr. Heberden, though he does not interdict bleeding, yet disapproves of "large, repeated, indiscriminate abstractions of blood, which experience proves to have been often prejudicial." Dr. Kirkland says—"It may be observed, that a loss of blood, to whatever extent carried, affords not any relief in the vehement apoplexy, and yet much dependance has always been had upon bleeding,—indiscriminately, in every disease which has been called apoplexy."—Dr. John Brown considers apoplexy as a disease of indirect debility, and therefore does not admit the evacuation of blood into his list of remedies, and Dr. Darwin says, "copious venesection must be injurious by weakening the patient."

Not to multiply instances I proceed at once to one of our most modern authors, in whose writings there is a very rational consideration of all the facts relating to apoplexy. I mean Dr. Copland, who says, in treating the subject in his Dictionary of Practical Medicine—"The treatment of apoplexy has long furnished subjects for discussion, not only as respects the more subordinate means of cure, but also as regards the most energetic measures, and the intentions with which they should be employed. This is evidently owing to the difference which has long been acknowledged to exist in the pathological states constituting the disease, but which has recently been questioned.—Without recurring to the changes so fully described above, I may remark, that a person is seized with apoplexy, and instead of being blooded is treated with stimulants and restoratives, and yet he recovers without paralysis having supervened. Another person is blooded largely and he recovers. A third is treated in a similar manner, and he becomes hemiplegic in the course of the attack, and a fourth is also blooded and he dies.—Now these are very common occurrences, and point to very important considerations which I will pursue a little further. A thin, spare and debilitated man staggers as he walks and falls down in the street, with pale countenance, feeble pulse, and

laborious or slightly stertorous breathing. He is blooded by the nearest medical man almost immediately and recovers. A large man of a full habit and lax fibre, suddenly becomes apoplectic, and is instantly treated with stimulants, and volatile substances held to the nostrils, and his consciousness and voluntary motion are restored in a few minutes. One practitioner of experience states, that he never draws blood from a patient in apoplexy, excepting under peculiar circumstances, and avers that he is more successful in his treatment than those who do. Another considers that when one full blood-letting fails of giving relief, no benefit will be derived from pushing it further, but much risque of giving rise to paralysis. A third physician, equally eminent and experienced, confides in blood-letting almost solely, and carries it often to a great amount, and a fourth, whilst he discards depletion, trusts to stimulants chiefly."

Such instances are sufficient to shew the variety of opinion that prevails on this important matter. The public is apt to consider bleeding by far the most powerful and appropriate remedy in this disease, and if a young practitioner was led by facts within his experience to doubt its efficacy, and had the firmness to act on this doubt, the feeling in favour of it is so universal that he would be likely to fall into some discredit if the case ended fatally. After the extracts I have given, I need not indicate how little support he would obtain for any opinion his experience had led him to, either for or against the practice if he chose to betake himself to study to escape his difficulties.

The pathological discoveries of the last few years have satisfactorily accounted for the marked difference that appears between writers of authority on this subject. All these differences would have been easily accounted for if it had been known that effusion—extravasation of blood—softening or hardening of the cerebral substance—abscess—deposition of false membrane, or almost any form of organic disease may exist in the absence of those symptoms which are usually supposed to characterise it, and that, on the other hand, every symptom may be present that is generally looked upon as indicating the presence of one of these conditions, and yet that condition be found wanting after death. Hence, while people were guided by symptoms alone, without considering, or indeed without knowing the extent of disease they might sometimes involve, and while bleeding was a remedy almost universally used on the sudden suspension of the

functions of the brain, it was no wonder this remedy should be found beneficial or injurious according as a less or greater extent of organic disease existed, and thus a physician's opinion with regard to this remedy would be favourable or otherwise, according as he happened to be thrown more frequently upon cases of the one class or the other. It is a result of later and stricter modes of enquiry in medicine, that symptoms cannot be depended upon as indicating the particular place or the actual amount of any cerebral disease. It is possible that when these methods have been persevered in for some time our powers of distinction in these matters may be strengthened; but though the mass of cases of this class on record is so abundant as to be absolutely overwhelming, the facts of each case have not been noted with sufficient accuracy, and more particularly, they have but seldom on the whole been followed by examinations after death, on which alone the certainty of our conclusions rests. It must, therefore, be confessed, that however desirable it may be to pronounce upon the existence of any particular pathological condition during life, there is, really, no means of doing so. The distinctive marks of particular states do not even amount to strong probability, much less to that well grounded conviction that would justify the use of powerful remedies, applicable, perhaps, to no more than one variety of organic lesion and very injurious in all the rest. The more modern writers, who fully admit all this, still continue to enumerate certain groups of symptoms as strongly characteristic of particular states. I need not say how useless this is, while they admit, as they almost always do, the uncertainty of the conclusion towards which they are driving. It is even mischievous, because such groupings of symptoms, when unaccompanied by statistical returns, have always a greater weight than they are entitled to in the mind of the medical student, which is directed more to the point to which they tend than to the caution by which they are accompanied. Besides, there is a source of uncertainty which is not hinted at at all. As far as I have been able to observe, the symptoms said to be indicative of a particular state are collected rather from a consideration of the functions such a state might be supposed to interfere with, than from facts drawn from cases in which that condition only was found present after death. This arises from the circumstance alluded to above—the small number of cases in which all the facts before and after death have been observed

with sufficient accuracy to found conclusions on them, and is only blamable inasmuch as it occurs with persons who admit the value of the numerical method and the uncertainty of mere opinion, and yet, in this instance, seem to forget their conclusion and fall into the old error again.

In directing attention to the utter impossibility of inferring, from any group of symptoms, the precise seat or the degree of any cerebral disease, I am sensible that I only point to a difficulty without shewing the means of escaping from it; yet, there is one conclusion of immense practical importance that I think will be found correct, namely, that in considering the expediency of bleeding *the actual degree of organic disease that exists* is of infinitely more importance than its seat or nature—in other words, that bleeding is badly borne in all cases of extensive organic disease of brain, *wherever the disease may be situated or whatever be its nature*. I do not deny that situation may be of much consequence in questioning the value of this remedy—our knowledge of the functions of some parts of the brain would tend to shew that it is so—but we are not yet in possession of facts sufficient to prove its relative importance, and even if we should be able yet to ascertain this, the tendency of such a discovery would only be to narrow the limits of those cases in which bleeding is useful. The great mistake made about bleeding in diseases of brain is looking to the fit as the test of its utility, instead of looking to the whole course of the disease and the event, and comparing cases in which it is used with those in which it is omitted, in all their circumstances, more particularly the last. Nothing can be more obvious than that the duration of the fit is a most fallacious test. There can be no doubt that patients would recover from the fit itself in all ordinary cases, whether they were bled or not, and even in many severe ones its utility if not its necessity admits of great question. What is it, I would ask, that produces the fit, and then the speedy recovery in many cases that occur in the street when a man falls down apoplectic, and in a few minutes regains his senses and walks home with or without assistance? If it be said that it is a congestion in some part of the head, it is plain that nothing could be more unfavourable to the occurrence of such congestion than the position the man was walking in, and that when he has fallen down nothing can be more unfavourable to its removal than his new position. How often does it really happen, notwithstanding all

the haste with which the medical man is summoned to the bedside in these cases, that he overtakes his patient in the very depth of the fit? Nay—how very seldom does it happen that the patient has not recovered some degree of consciousness even before the physician can resort to bleeding, though he has been laid in bed in a position more favourable to congestion than almost any other. To take a parallel case. We know that the action of the heart and arteries depends for its continuance upon the performance of the respiratory function—yet, what should we think of a person who would assert that every excitement or suspension of the circulation depended upon changes in the respiratory function. In this case we are able to disprove the assertion by shewing that the respiration is often quite unchanged amid these changes. In the other we have, it is true, no means of proving that there is no extraordinary congestion in any part of the brain, but the assumption that every alteration or suspension of its functions depends upon an altered condition of the cerebral circulation, will, I think, be considered fully as extravagant upon a fair consideration of all the facts connected with the fit and recovery in such cases. It ought never be forgotten that though a certain condition of the circulation in the brain is essential to the manifestation of its functions, yet, when once its action is set up, there is a certain independence about it, and its functions are exercised and capable of undergoing exaltations and depressions subject to other stimuli, though the cerebral circulation may maintain an even current, and it would be as absurd to say, that every expression of the feelings in the countenance—every look of joy, or sorrow, or contempt, or anger—itself a manifestation of the exercise of the proper function of the brain, was dependant upon an altered state of the cerebral circulation, as that every other exaltation or depression, or even suspension of its functions, was *always* so. I may appear to have entered into these observations at unnecessary length, but I do so, because I think the assumption that changes in the state of the circulation or “determinations of blood to the head,” as they are called, will account for most of the interruptions of function in the brain is a very dangerous error, and I am convinced that many persons have been over bled in consequence of it. The cause of the error is manifest. There are many cases of apoplexy in which large bleedings are not only useful but highly efficacious. Most of these, I suspect, are

cases arising from affections of very recent origin, in which but little damage or disorganization has been effected in the brain.—The organ has undergone little if any structural alteration, and bleeding will be borne as well in them as it could be in the most healthy person. The advantage derived from it in these cases is so manifest, that the remedy is extended to all. If mischievous effects follow they are attributed to the inevitable progress of the disease, and thus the error is perpetuated. A case that bears a remarkable resemblance to these is the one of poisoning by opium. Here we have a perfect apoplexy, as far as symptoms are concerned—stertorous breathing—insensibility—immoveable pupils—a laboured circulation and imperfect oxygenation of the blood—but the brain is uninjured by disease, and accordingly bleeding will be borne in such cases to almost any extent and with remarkable benefit. This seems to strengthen the conclusion I am endeavouring to enforce—that the amount of disorganization is the principal source of danger in the employment of this remedy. I need not advert again to the numberless cautions that are given against the indiscriminate use of it. Some of them I have quoted in the beginning of this paper, and I observe that even the most strenuous advocates of what is sometimes called a heroic* practice, make admissions occasionally that would lead one to suspect them of having fallen into the errors of the French school, by founding their curative measures more upon appearances observed after death than upon the circumstances that followed the administration of remedies during life. Thus, I find Dr. Abercrombie—one to whom the profession is indebted for the first specimen of any thing approaching to strict investigation in these diseases—saying of purging, “This is always to be considered as a most important and leading point in the treat-

* I cannot help noticing the absurdity of the term “heroic,” as applied to these things. We hear of “heroic bleedings,” “heroic doses of calomel,” &c. Our notion of heroism always includes some danger—often a great degree of it; but we are not in the habit of giving any man credit for heroism in a transaction of which all the danger is borne by another party. The physician, however much identified with his patient in feeling, is certainly not so in person or constitution, and he cannot therefore be entitled to all the glory arising out of the administration of scruple doses of calomel or bleedings of forty ounces at a time. The expression is, I believe, of French origin, and seems to be caught up in these countries as a happy one. I observe it is made use of by medical men only. Quere—would their patients think it equally happy?

ment of apoplexy; and though, in arresting the progress of the disease, our first reliance is upon large and repeated bleeding, *the first decided improvement of the patient is generally under the influence of powerful purging.*" Surely this is an admission of immense importance. The cautions given by medical writers on this subject, though numerous enough, will be always ineffective so long as they are unaccompanied by any principle by which to shape our course. The rule that would suggest itself is—to examine in all cases what evidence there is of the existence of much organic disease, and if there is reason to suspect that it is present in any considerable degree, to be very cautious in the abstraction of blood. Indeed, where loss of power is already apprehended, the system should never be so much lowered as to interfere with the process of reparation. It ought also be a rule to trust rather to moderate bleedings repeated at proper intervals than to run any risque of inducing debility in the after part of the case by taking a large quantity at once. Indeed it appears to me that the effort ought always to be to reduce the circulation to its natural condition, and that it is by no means a direct way of attaining this end to run into the opposite extreme, and resort to large bleedings by way of making, as it is said, a powerful impression. The danger attending such practice and the necessity of the cautions I have given will be evident from the following case which was given me by a friend, and is one of the most remarkable I have met with.

Mr. —, a strong, stout plethoric man, suffered for two or three years from frequent returns of secondary syphilis. The skin had been repeatedly covered with eruptions—spots had ulcerated in several parts of the body, forming deep sores—the throat had been ulcerated—several of the joints successively inflamed. The head and shins had been attacked with painful nodes—the nose had been also attacked, but the affection was for the time arrested. Within the last eight months, after a long interval of tolerably good health, the eruptions on the skin broke out again, and he complained occasionally of pain in the head, which was supposed to be a return of the old syphilitic pains. It was followed after some time by giddiness, deafness particularly of one side and noises in the ears. The giddiness and deafness were very much relieved by the treatment made use of to remove the eruption, and still more afterwards by taking spirits of turpentine and purgatives. As he had a strong objection to cupping and blistering,

which was proposed to him, and the turpentine in doses of thirty drops three times a day, with an occasional full purgative and low diet, seemed to give him progressive relief, they were continued until he declared himself quite well, which was about six months from the first complaint of giddiness. Soon after this, he was observed one day to get confused and wander a little in conversation, and fell back off his chair in convulsions. He worked violently for some minutes—then became easier, but breathed loud and stertorously and frothed much at the mouth. The convulsions returned every five or ten minutes for ten or twelve turns, although over thirty ounces of blood had been taken from his arm at one bleeding. At the end of two hours the convulsions ceased, and sensibility was so far recovered that he was capable of some voluntary movements and could articulate. He now began to roar or scream, strike his arms about, and twist his body violently, so that five or six men were necessary to hold him. As this violence was continuing in a frightful way for some time, and his pulse was good, ten or twelve ounces more of blood were taken from the arm, soon after which he became quieter, and his pulse was soft at 90. He was evidently sensible to pain and swallowed a spoonful of water. He now got ten grains of calomel and soon after a purgative enema, which seemed to empty the rectum, but no more. In the course of another hour there was a very considerable amendment. He recovered his consciousness perfectly, swallowed well and became quiet. He had no recollection or knowledge of anything that had happened—wondered at the fuss and work that was about him, and asked why his head had been shaved and cold lotions applied to it. His pulse was over 100 and rather weak than otherwise, his bowels had not been moved except a little by the enema. Another, therefore, was ordered, and he got a scammony powder by the mouth. Another was ordered to be given in three or four hours, and the enema to be repeated unless the bowels were freely moved. At night when I saw him again he was quite easy and his senses perfectly restored—but I observed a very disagreeable quickness of respiration about him, and his pulse was yet weaker than before at 120. A blister was applied to the back of the neck, and directions were given if he got any weakness during the night to give him wine and water. Soon after leaving him (as the apothecary informed me in the morning) the pulse became still fainter, and he grew very restless. He got no sleep, but tossed and turned i

the bed the whole night—perspiration broke out on his forehead and face—his extremities cooled, and his pulse was scarce perceptible at the wrist. When we saw him early in the morning we found his bowels had been moved freely through the night—he was perfectly sensible but pulseless and anxious and his respiration extremely rapid. He could not be kept quiet or in one position for three minutes together, but was continually flinging himself from one side of the bed to the other. He was, in fact, precisely in the condition of a person dying of hemorrhage, and presented a striking resemblance to cases which I had seen of women with severe flooding after parturition. He got five grain doses of opium, brandy and broth, without the pulse returning to the wrist or the least excitement of the circulation, and expired in about two hours!

The close of this case was very remarkable. The apoplectic symptoms passing off so completely and no others being present towards the end, except signs of pure debility, shew that whatever the nature of the cerebral affection may have been, (for there was no examination after death), the bleeding, if useful at all, was certainly carried farther than necessity required. Yet, it was a case in which, according to the usual modes of thinking, no one would hesitate—a large, strong, and temperate man, with a substantial and full pulse, suffering under an affection of the brain, sudden in its occurrence and violent in its symptoms. These circumstances are of immense importance, as the same signs of debility and the same result following a bleeding in a spare frame, only moderately strong, would not have furnished so forcible an inference against the excesses of this practice as such an inference would then be partly accordant with our *a priori* reasoning.

In giving the estimated amount of organic change as a guide in determining the degree to which bleeding is to be carried, I, of course, include all affections of brain dependant upon organic changes, whether apoplectic or not. There are, however, other circumstances in which cautions against the use or abuse of this remedy are exceedingly necessary, even though the amount of structural alteration may not be considerable. It appears to me that all affections of the brain or its membranes attended with protracted suffering are cases which bear bleeding very badly. I believe every physician of any experience must have met with instances to convince him of the truth of this—and it is a result

one would readily anticipate. Some physiologists assert, that the vital functions are mainly dependant upon the sensorial for their exercise. Whether this assertion be true or not, it is quite certain, that a violent and sudden impression on the sensorium is capable of producing such a sinking of the vital powers that, in some instances, the functions of life will cease at once. Many facts of this kind are on record—a violent and excruciating pain has been known to produce this effect; and it seems to matter little whether the impression produced be one of pain or of pleasure provided it is sufficiently strong. The story of the Grecian who expired suddenly on being told that his children had won at the Olympic games is a case in point. The effect of such impressions on the nervous system might be pursued much further, but it is sufficient to remark, that the same exhaustion that a very violent and unexpected impression will produce at once will be produced by a less violent one often repeated or long continued, such as the pain attendant upon any acute disease that occupies some days in its course. I may mention as particular instances those cases of suppuration within the head, in which the symptoms of inflammation often run very high towards the close. In these circumstances, and indeed in all cases of whatever kind in which excruciating pain is a prominent symptom, (for the remark I think applies generally,) whether there is little organic change or none at all, or however violent or high the symptoms may be—if this state of things has lasted many days free bleeding will not be borne. In fact, long continued agony independent of anything else produces of itself a degree of direct debility, which, however, it may be masked by inflammatory symptoms, truly exists, and is often as great as if the patient had already lost several pounds of blood. The manner in which death takes place in such cases would lead one to the same conclusion. If we compare their termination with that which marks some of those examples of cerebral disease that occur in full habits advanced in life, that commence with drowsiness and lethargy, almost without any suffering, and end in the gradual supervention of coma, we shall find the difference very remarkable. In the former death often occurs suddenly and unexpectedly, and in many instances long before the symptoms of pressure from accumulated matter have shewn themselves. In others these symptoms have appeared but partially, and the patient retains many of his mental faculties to the hour of death. In

others still the patient is perhaps talking to his friends one moment and in the next lays his head on the pillow and dies. In the latter class this is never the case—whether the progress of the case depends upon the gradual accumulation of fluid, or something equivalent to it, there is little or no pain and therefore no exhaustion—death takes place very slowly—function is interrupted after function—the eyelids, cheeks, muscles of the tongue, and soft palate, are paralyzed in regular order—the powers of sense and voluntary motion are entirely obliterated, and last and latest of all the respiratory movements—the former are instances of death by exhaustion—the latter by interrupted function. I shall give a case of the former class, furnished to me by a medical gentleman of much ability, in which the symptoms seemed intense, the bleeding moderate, and yet the result was fatal—

“ A man, aged 40 years, was seized with most intense pain in the head which continued for several days to a degree that nearly drove him distracted. No medical advice was procured for him until the fifth day, when he was found by the physician then called in, with a bewildered unconscious look, but still groaning with pain—he had grinding of the teeth, and the pupils of the eyes were contracted—his pulse was quicker than natural but not weak, and his stomach retained everything which he drank. The case appeared to the person attending a very hopeless one, but as the only alternative offering a chance of benefit, he took twelve ounces of blood from the temporal artery—shaved his head, and poured a kettle of cold water over the crown and occiput. After directing a blister to be applied to the nape of the neck and the administration of some purgative, he left the house; but had not gone a quarter of a mile from the door when the man expired.”

Let me conclude by recapitulating the circumstances in which I consider large bleedings improper with the principles on which they are so :

1.—They are improper, because symptoms are no certain test of the amount of disease, and we may produce a degree of debility (as in the case given) that cannot be contended with.

2.—They are improper in all cases in which extensive disease of brain is known or suspected to exist, as in such cases besides the immediate danger, they produce a degree of debility that would interfere with the process of reparation so far as that is possible.

3.—They are improper in all cases of disease of brain attended with severe and protracted pain, as such cases usually die, not from any mechanical effect of an existing inflammation, but from the exhaustion produced by the pain that accompanies it.

As a general rule I would, therefore, even in circumstances in which the loss of a large quantity of blood was supposed to be necessary, prefer taking it away in moderate quantities at certain intervals, watching the progress of the case, and being guided entirely by it.

PROBLEM IV.

BY WILLIAM GRIFFIN, M.D.

ON WHAT MORBID STATE DOES THE OCCURRENCE OF COMA,
AND SUDDEN DEATH IN JAUNDICE DEPEND ?

A POOR woman requested me to visit her daughter, Mary Barry, aged twenty years, who, she informed me, had been three days ill, and was now speechless, and she believed dying. On entering the cabin in which she lived, I saw her make a faint expiration, which proved to be her last, as she was quite dead when I reached the bed. Her skin was still warm, and universally tinged with a deep yellow colour. The countenance was hydroptic, and the pupils dilated. On inquiring, I found the girl's ailment had set in with langour and heaviness ; on the second evening she was seized with sickness of stomach, vomiting, and appearances of jaundice, and next morning complained much of her head. She then looked so very ill, that her mother began to get alarmed, and insisted on her going to the dispensary for advice ; the poor girl shook her head despondingly, and said she was too weak to walk there, but that she would go into the room and lie down on the bed. These were the last words she uttered ; when the mother went in afterwards, there was an appearance of stupor about her, from which she endeavoured to rouse her, but could get no reply. She was in profound coma !

In about three weeks after, I was called to see Ellen Barry, a sister of the former, and found her labouring under an affection precisely similar. She had been attacked with langour and heaviness, followed by sickness of stomach and vomiting, with universal yellowness of the skin. She was now in imperfect coma ; conscious when roused, but unable to speak, and very unwilling to be disturbed. From this very dangerous state she was rescued by active and continued purging ; the yellow tinge gradually disappeared, and in a few days she regained her usual health.

Within a very short period afterwards, another member of the same family was attacked ; a boy, of about 13 years of age. My brother was requested to see him, and found him moaning and comatose ; his belly tender to the touch, his pulse slow, and

his skin of a saffron colour : his breathing was not stertorous.— This case was more sudden than either of the foregoing ; the boy was seized with sickness of stomach and vomiting at night, and in the morning was jaundiced and insensible. In this state he lay, until nearly the end of the second day, without medical aid, up to which period his bowels had not been moved. An ineffectual effort was then made to purge him, but he was unable to swallow, and died in a few hours.

The parents were now, it may be supposed, highly apprehensive for their remaining children, and the event proved not without just reason. After the lapse of a few months, their next boy, John Barry, aged eleven years, shewed symptoms of jaundice. He grew languid and heavy, and in two days the tunica albuginea and skin were of a deep yellow. There was a great sluggishness of the bowels, and slight tenderness of the abdomen, but very little pain. He did not complain of his head, but like the others, was seized with sickness of stomach and vomiting. I had early notice of this attack, and was vigilant in looking for the supervention of coma, although from any existing symptoms there was no greater reason to apprehend it than in any common case of jaundice, if I except some slight dilatation of the pupils and sluggishness in their movements. The boy was up and about, and did not in fact appear to be very ill ; but the fate of his brother and sister left a lesson not to be forgotten, and I accordingly warned the mother to give me instant notice on the occurrence of the slightest stupor,—he was in the mean time actively purged. There was little change in him that night or the next, but on the succeeding morning I had a messenger with me at an early hour, to say that he had fallen into a state of insensibility in the night, and could not now be roused. I found him quite comatose, with slow pulse, dilated pupils, and almost a total loss of sensation and voluntary motion. On pinching his hand severely, however, he evinced signs of consciousness, moaning slightly, and slowly drawing his hand away. Ten ounces of blood were immediately taken from the temporal artery ; the head was shaved, and kept wetted with refrigerent washes, and castor oil was administered every fourth hour. As the bowels were slow in acting, injections were given at night, and large blisters applied to the nape of the neck. These had the desired effect. He was copiously purged for several hours, and in the morning evinced signs of returning consciousness ;

from thenceforward there was, day after day, a steady and progressive improvement, until his recovery became fully established. Some time after his friends were once more alarmed by a recurrence of the vomiting and jaundice : but the progress to coma was arrested, and the complaint readily removed, by full purging alone.

These four cases of jaundice running rapidly into coma, which in two of them terminated in death, when we consider that they occurred in one family,* within a few weeks of one another, and without any unusual or remarkable symptoms which could indicate the impending danger, suggest a very important question with regard to the pathology of the disease : “ On what morbid state did the occurrence of coma in these particular instances depend ? ”

In referring to the works of different authors who have written on the subject of jaundice, it surprised me much to observe, that the occasional supervention of coma and sudden death is scarcely adverted to. This is not noticed even by Cullen or Parr, as a possible termination of the complaint, nor is there any mention made of it in some of the more modern medical treatises,† a circumstance perhaps scarcely deserving of remark if the occurrence was really rare. We might offer many evidences to prove that it takes place too often to be left at any time out of view in our consideration either of the prognosis or treatment of the disease. Mr. Gilbert Burnet and Dr. Macleod, in a discussion on the subject three years since at the Westminster Medical Society, detailed several cases, which, with few appearances indicative of danger, ran rapidly to coma and death,‡ and those published

* I have been inclined to think jaundice is sometimes occasioned by certain states of the atmosphere, from its now and then attacking many individuals in the same locality. I was myself suddenly affected with it some years since in common with my servant and many others in the neighbourhood in which I then resided.—These were all under empiric or regular treatment, and recovered in four or five weeks. I took no medicine, except an occasional mild aperient, lived on roasted apples, almost the only food I could use, and was well in three weeks. The cause of the disease is frequently so obscure, that we really do not know what value to attach to medicine, even where recovery takes place.

† Dr. Mason Good speaks of the occasional occurrence of apoplexy in green jaundice only. Dr. Mackintosh, in his *New Practice of Physic*, makes no mention of it ; and the writer on jaundice in the *New Cyclopædia of Medicine* is equally silent.

‡ See *Medical Gazette*, vol. v. p. 631.

by Dr. Marsh in the Dublin Hospital Reports, illustrating the occasional connexion of jaundice with disease of the brain, were probably of a similar nature.* Dr. Gregory, of London, calls our attention to the probability of such terminations in severe cases, and Dr. Mason Good speaks of the supervention of apoplexy in green jaundice, chiefly instances where the pulse was unusually slow. But it is neither in severe instances of the disease, nor in that intense form of it which has been called green, that it most frequently occurs; and nothing so clearly proves how little we really know of the pathology of these affections, than the fact, that the probability of the supervention of coma bears no relation to the intensity of the symptoms. In Dr. Macleod's cases there was little to indicate danger, until fatal coma occurred in one, and epilepsy, followed by coma and convulsions, in the other. In three of the instances which have preceded these remarks there was nothing that could lead one to anticipate immediate danger of any kind, until actual stupor commenced: they were not cases of green jaundice, and previous to the occurrence of fatal symptoms could not even be called severe or unusual.

The general connexion existing between jaundice and certain affections of the brain or nervous system, which attracted the attention of physicians at a very early period of time, and to which the occurrence of the disease from intense passions of the mind has been attributed, while it tends to diminish our surprise at the occasional occurrence of coma, furnishes no clue to the subsisting relation; several explanations have from time to time been offered by those who have paid any attention to the facts, but none that are at all satisfactory. Unfortunately no post mortem examination was permitted in any of the fatal cases which I have mentioned. In one of Dr. Macleod's, that of a young woman who had jaundice for some time, without suffering much inconvenience from it, but who died in forty-eight hours after the supervention of coma; the only morbid appearance observed in the brain appeared to be, a deep yellow colour of all the membranes.

Although the yellow colour of the skin and eyes has in these cases been always the first circumstance to attract attention, the mutual sympathy, which is known to exist between the brain and the

* Dublin Hospital Reports, vol. iii.

liver, has led to a very natural doubt as to which might be the primary seat of disorder, and in fact, in this lies much of the difficulty experienced in endeavouring to explain its pathology. Those who have considered the affection of the head as secondary or symptomatic, have attributed it to a plethora of the circulation of the brain, occasioned, like the jaundice, by a gorged state of liver, equally obstructing the passage of blood and bile, or to a superearbonization of the blood, for want of due elimination by the liver, or to a highly azotised state of it from the same cause, the brain being affected in either case as it is in apoplexy from the circulation of venous blood, or lastly to some such sedative effect of absorbed bile on the cerebral organ, as may be induced by opium or other narcoties. On the other hand, those who have considered the jaundice in these instances, as secondary or symptomatic only, have supposed some oppressed or actually diseased state of the brain, making for some time an insidious progress, and at length manifesting itself by suspending the functions of the liver. An accumulation of bile in the blood vessels, it is said, takes place as a necessary consequence, and precedes, though it be no way necessary to, the termination in coma, which is simply the conclusion of the original affection.

Glancing at these conjectures in the order in which I have stated them, it may be remarked, that no such gorged or infarcted state of the liver has been made out in the cases alluded to, while, I believe, in others where such condition of that organ did exist to an extreme degree, there was no supervention of coma; and again, one should suppose, where the danger depended on pure plethora, the complaint would be easy of remedy, which is by no means the fact. It might, perhaps, be said, this idea of a plethoric state of the circulation in the brain, derived some countenance from the two recoveries quoted, one by pure purging, the other by purging, bleeding, and blistering: but these remedies are equally applicable to other morbid conditions of that organ.

If the elimination of carbon and azote from the blood be one of the chief offices performed by the liver, its total suspension must necessarily lead to a loaded and deteriorated state of that fluid. That such a state occurs to a certain extent, in almost all cases of jaundice, we have manifest proof in the general languor complained of, the slowness and feebleness of pulse, and altered nature of the secretions. That it sometimes leads to actual coma

and death, has been already shown, and that the presence of bile in the circulating system, when artificially introduced, produces analogous results appears from Mr. Phillip's experiments, in which two drachms of bile, injected into the femoral vein of a dog, in a few hours occasioned jaundice, dryness of mouth, vomiting, coma, and death, and in a lesser quantity, effects of the same nature, though less marked.

It would seem superfluous to seek for other proofs of the injurious effects of retained bile, if those I have instanced were any way constant in their occurrence. But that they cannot be received as such, and that they are rather the exception than the rule, must be obvious, when we recollect, how many persons, in whom every texture of the body is deeply imbued with yellow bile, while not a particle passes into the intestines, live for months and years without suffering much inconvenience,* and how infants have grown rapidly, and thriven tolerably, where the hepatic ducts were altogether impervious.† It is clearly unphilosophical to attribute effects in one case to a cause which in nineteen others seems incapable of producing them; but setting these considerations aside, and viewing the question physiologically, there is, after all, no just reason for inferring a supercarbonized or azotized state of the blood, from the non-elimination of bile. Tiedemann and Gmelin have made it appear exceedingly probable, "that the pulmonary and biliary organs are, in different tribes of animals, nay, even in different individuals of the same species, in a state of antagonism to one another; that the size of the liver and quantity of bile are not proportionate to the quantity of food and frequency of eating, but inversely proportional to the size and perfection of the lungs." That in fact, as a

* Dr. Gregory mentions that he has seen young persons continue busily engaged in an active employment, their appetite, sleep, pulse, and tongue, remaining healthy, where yet the jaundiced colour of the skin was intensely deep. It was the experience of this fact, probably, that induced Dr. Fordyce to imagine, that the bile was of no use whatsoever in digestion.

† Sir Everard Home has given an example of a child that fed heartily, seemed to digest its food well, and had regular stools, which was, nevertheless, without a gall bladder, or even a duct of any kind leading from the liver to the duodenum. Dr. Blundell records the case of two infants, four or five months old, in whom the hepatic ducts terminated blindly, so that no bile entered the intestines: the stools were white, like spermaceti, and the skin jaundiced; but the infants had grown rapidly and thriven tolerably notwithstanding!

secreting organ, the liver is chiefly excrementitious, assisting the lungs and cutaneous surface in decarbonizing the blood, and consequently, when interrupted in its function, the duty is merely transferred to the latter, which immediately take on an increased action. To that beautiful relation and correspondence subsisting between all the organs of the body, and especially those engaged in nearly similar functions, we are indebted for the impunity with which we can occasionally suffer the temporary suspension of any of them, and in no instance is this more strongly illustrated than in jaundice, in which the interruption of the hepatic excretions, those of carbon and azote, is met by increased decarbonization in the lungs and skin, and increased excretion of highly azotised principles by the kidneys. Although it seems improbable, that a poisonous state of the circulating fluid can occur in this manner, the occasional consequences of jaundice necessarily countenance the less definite conjecture, that bile retained in the circulation acts at all events in some way or other, as a sedative on the brain and nervous system. To this idea may be traced the frequent application in practice of many influential remedies, and the still more frequent attempt at explaining the most striking phenomena in very obscure affections, by referring them to obstruction in the liver. It is clear, however, we can as yet go no further than to admit some connexion between these effects and their supposed cause, as they bear no regular proportion to it in their intensity, and are anything but necessary results. We know no more why sedative effects should result from obstruction to the flow of bile in some instances, than we do of their total absence in others.

An endeavour has been made to draw some distinction between cases of jaundice in which the bile is not eliminated by the liver, and those in which it has been secreted and re-absorbed. That such distinction exists, and that the former are of a more dangerous nature than the latter, inasmuch as they necessarily include either paralysis, or great disorganization of the organ, no one can deny ; but it does not follow from this, that the system sustains more injury by the want of elimination of bile, than by its secretion and absorption. The cases usually end fatally, not because the blood is more vitiated, but because the vitiation, such as it is, arises from, and is accompanied by more serious disease. If, then, we cannot account for these cases of sudden coma, by any absolute effects of retained bile, it only remains

for us to inquire whether they might not be explained on the supposition of previous cerebral disease.

There are very many interesting facts, which would tend to shew, that the brain is, in some instances at least, the organ primarily in fault in jaundice. Besides the well known occurrence of abscesses, and other diseased states of the liver from injuries of the head, sudden yellowness of the whole person has not unfrequently followed intense mental emotion, and has often been observed in fevers and other diseases, in which the brain and nervous system have been much affected. Some of the cases published by Dr. Marsh, to which I have already adverted, seem to have depended on an affection of liver, and of the mucous coat of the intestines, originating in cerebral disease. If, in such instances, we could suppose the affection of head to be so obscure, as altogether to escape the attention of the practitioner previous to the occurrence of jaundice, there would be little or no indication of an unusually dangerous form of the disease. He would almost necessarily attribute the headach, languor, and sickness of stomach to the retention of bile in the circulation, and the supervention of coma and apoplexy would seem sudden and unaccountable; when, if he could have suspected the source of the disease, it would have been anticipated as a very probable termination.

We have not, unfortunately, a sufficient number of reports of post mortem examinations in those cases, to form any decided opinion on the subject. If, with such imperfect materials, even a conjecture might be hazarded, I should, on the whole, be disposed to say, that the cerebral affection is rarely the primary disease, but is superinduced, we know not how, by the suppression of a most important excretion, as it sometimes is in the suppression of the catamenia, and almost always of the urine.—When we find inflammation of the brain or its membranes suddenly brought on by obstruction of the uterine or renal discharges, we cannot be surprised that a suppression of one of the most important in the whole system, whether as a secretion or an excretion, should occasionally induce it. That the occurrence of coma in jaundice generally depends upon some such state, suddenly induced, and not upon previous or long standing cerebral disease would seem very probable, from the success of the treatment, and rapid recovery of two of the cases reported, and from the fact, that in the only one of Dr. M'Leod's in which

there was an examination after death, no disorganization of the brain was discovered. I cannot tell why cerebral inflammation should arise from obstruction to the flow of bile in one instance, and occasion no such result in a hundred others, any more than I can account for its somewhat rare occurrence in suppression of the menses. These observations are offered, however, not with a view to the solution of the difficulties which have been pointed out, but as a faint light to the practitioner, until the question can be investigated with some prospect of success.

PROBLEM V.

BY D. GRIFFIN, M.D.

IS THE LAW OF VISIBLE DIRECTION, AS AT PRESENT RECEIVED, A TRUE ONE?

THE controversy as to the cause why an inverted image on the retina gives us a perception of the object looked at in its erect and natural position, has, it is well known, been terminated by the discovery of certain properties in the retina itself, which were said to be resolvable into a remarkable law, that afforded a perfect explanation not only of this, but of many other circumstances regarding vision. These properties were believed to be an original endowment of that membrane, and impressed on it for the purpose of connecting our perceptions of the objects that surround us with their real positions, directions, and other qualities appreciable by sight, in a uniform and simple manner. This law is expressed by saying, that "whenever rays proceeding from any point of an object are brought to a point on the retina, the whole of these rays, or any of them, will represent that point of the object in the direction of a line perpendicular to the part of the retina on which they fall, without any regard to the degree of obliquity with which they may have fallen on it. It had its origin in the observation of the following circumstances, which I give particularly, that, in denying its existence, I may not be thought to underrate their importance:—

1. If we make moderate pressure with a blunt point on the ball of the eye, a black spot, surrounded in most cases by a luminous ring, will be seen; and this spot will take up a position opposite to the point impressed.

2. As the pencil of rays by which any point of an object is seen has its greatest breadth at the cornea, converging from thence in two cones towards the object and the image on the retina, we might expect that, by shutting out all of the pencil except a few rays which pass in near the margin of the pupil, the object would seem to shift its place, since in this case we see it by means of rays which come to us by a circuitous course, and of which no portion that pierces the eye points directly from the part of the retina impressed, towards the object. We shall find,

however, under these circumstances, that the object keeps its position, and is seen in its real direction as perfectly as if we had admitted the whole pencil.

3. In following the course of a pencil of light through the eye, it will be found that pencils entering it at angles of 45° and upwards from the axis, do not contain a single ray which points directly from the part of the retina impressed towards the object; yet it is well known that objects at such angles, though indistinct from other causes, are seen in their true directions.

4. Another argument in favour of this property may be drawn from considering the situation of the punctum æcum, or blind point of the retina. Objects below a certain size become invisible some distance to the right of the axis in the right eye, and to the left of the axis in the left; whereas the entrance of the optic nerve, on which this defect depends, takes place to the left of the axis in the first case, and to the right of it in the second.

Other facts are mentioned of the same nature and tendency; but the most remarkable, and—simple and beautiful as it is—by far the most important of all, in confirmation of the existence of this property, is the following experiment of Schciner:—

If we take a small object, the head of a pin, for example, and bring it before the eye some distance within the best point for distinct vision, we shall see it enlarged, but hazy and indistinct, the rays from it being too divergent to be brought to a point on the retina. If we now place a card, perforated by a small pin-hole, between our eye and the object, a portion of that divergent pencil will pass through the hole, the rays of which are so nearly parallel that they will be refracted to a point. We shall now, therefore, see the head of the pin magnified and perfectly distinct; but the remarkable circumstance is, that, keeping the pin steady, if we move the card up or down, or to the right or left, which movements, of course, change the place of the image on the retina, the object will appear to move in an opposite direction in each case, though it shall not actually have changed its place at all.

Thus the first-mentioned facts prove that obliquity of incidence in the rays does not effect any apparent change in the place of the object, provided they still fall on the same point; while Schciner's experiment proves that when this point is varied, there is an apparent change of place, and that this apparent

change its in a direction opposite to that in which the image on the retina is made to move.

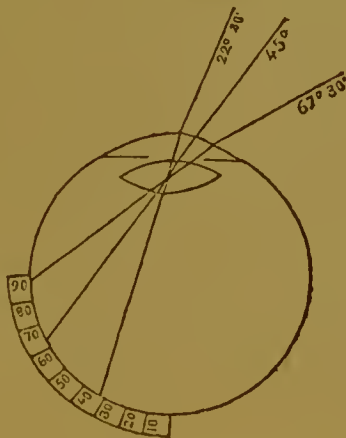
An interesting variation of the experiment, and one which leads to the same conclusion, is the following :—If we make two additional pin-holes in the card, one at each side of the former, and just so near it that the rays from the pin head through the whole three may pass through the pupil together, by holding it between our eye and the object we shall now see, as it were, three pins' heads. If any one of the holes is stopped, one of these will disappear ; and by trials we shall find that, when the hole at the right side is stopped, it is the left pin that disappears, and *vice versa*.

I have been thus particular in stating the law as at present received, with some of the principal facts on which it rests, because I think some circumstances have been overlooked which are utterly incompatible with its existence. A few years ago, being engaged in delivering some public lectures of the most elementary kind, on optics, at the Limerick Institution, it fell to my lot, during part of the course, to describe the structure of the eye, and its action as an optical instrument. In drawing the necessary diagrams, some circumstances occurred to me, which impressed me strongly with the conviction that this law could not be true. As, however, any discussion on the question would be out of place before an audience, many of whom were not familiar with the subject, I gave the explanation which is now generally received—a very unpleasant task to execute while I had the feeling of its fallacy so strong upon me. My attention has been drawn to the subject from time to time since ; and though during that period I have seen many writers receive it as if it was well established, yet of this at least I am now certain, that the above law cannot be true under the conditions specified.—These conditions are, that the concave surface on which the retina is expanded is spherical, or nearly so, and that the curvatures of the different humours of the eye, and their indices of refraction, as given by the best authorities, do not differ widely from the truth.

The first of these conditions has, I believe, never been disputed ; with regard to the second, the refractive indices and curvatures of the different humours have been examined by M. Petit, Dr. Young, Brewster, Gordon, Wollaston, and others ; the following are the results ; such of them, at least, as are necessary for our present purpose :—

Interior transverse diameter of the eye	·90
Horizontal cord of the cornea	·49
Radius of external surface of cornea	·33
Radius of anterior surface of lens	·33
Radius of posterior surface of do.	·24
Distance of iris from cornea,	·11
Distance of lens from do.	·12
Refractive index of aqueous humour	1·337
Refractive index of lens (mean refraction)	1·384
Refractive index of vitreous humour	1·339

Let us suppose the interior of the eye, on which the retina is spread, to be graduated from 0 to 90° , and so on, beginning from the point where the visual axis strikes that membrane, and marking this point zero. If now, using the above curvatures and refractive powers, the progress of a pencil of light be traced graphically, following the usual law of the sines, it follows that it the above law of "visible direction," as it has been called, is true, the number of degrees marked on the point of the retina where it is found to fall, ought to indicate the degree of inclination of the pencil to the optic axis before it entered the eye; that is, in fact, ought to indicate the actual direction, in space, of the object from which it came, or at least, according to Sir D. Brewster's version of the law, ought to indicate the position of a line parallel to it, which would come to the same thing. In tracing the ray thus, the mean refractive index of the crystalline may be taken, instead of its varying one, without affecting the course of the pencil in any degree worth speaking of. Indeed, it will be found that by far the greatest portion of the whole refraction that takes place in the eye is produced at the cornea, and that when this is accomplished, the refractions produced by the lens and vitreous humour have but little influence further on the direction of the ray. The accompanying figure is a copy of



a much larger one, drawn in conformity with these conditions ; and it will be found that, tracing the course of the ray in the manner described, a pencil inclined $22^{\circ} 30'$ to the visual axis, will fall somewhere about 34° on the retina ; one inclined 45° , will fall on a point marked 63° ; and a pencil inclined $67^{\circ} 30'$, will fall on a portion of the retina which, if it possessed the property above mentioned, must represent it as standing nearly 39° from the axis of vision. Pencils at intermediate stations will be found to deviate in intermediate degrees ; but in every case there is an error, which increases with the inclination of the pencil, though not exactly in the same ratio.

Now it is well known that exteriorly the field of view extends even 90° from the axis—that is—when the eye is directed straight forward, we can see objects in any position, from 0 up to 90° , on the outside of the eye, and see them in their true positions. But it will be found, by trials on a diagram such as I have described, following the same rule as for the other rays, that a pencil of light from an object removed 90° from the axis, cannot possibly be brought to a point marked 90° on the retina. The question is not here, whether we can see them distinctly or not, but whether we can become sensible of their presence, and if they are represented in their true positions ; and neither of these points, I believe, has ever been disputed.

Thus it is evident that this law, stated, as it has been, on what I cannot help calling a very loose examination of the circumstances, is not true, nor even nearly true ; and we do not get out of the difficulty by supposing that the indices of refraction of the different humours have been under-rated ; for (not to speak of the improbability that such a large error could be committed in ascertaining them as this supposition would require) it is clear that the refractive power necessary for bringing pencils entering the eye at angles of 45° , or 70° , to points at the same angular distance from the extremity of the visual axis, would make the eye myopic for pencils lying near the axis, and would thus render the very best part of the retina utterly useless. We cannot, therefore, put the facts we have collected under any general expression, but must rest satisfied, for the present, with the simple statement, that when rays of light fall on any point of the retina, that point has the property of representing the object from which they come *in its true direction*, without any regard to the obliquity of their incidence.

I have examined, with the assistance of another person, the positions of the spectra when pressure is made on different parts of the ball of the eye, in order to see how far facts of this kind support the view which is sought to be maintained. Extreme accuracy cannot be expected in such examinations, but the following are the results, as near as possible :—When the pressure is made 90° from the axis on the outside, the spectrum appears anterior to the bridge of the nose ; when the axis is directed towards this latter part, and pressure is made as deep as possible on the outside of the eye, the spectrum appears a little within the bridge of the nose ; when the axis of the eye is directed outwards as much as possible, and pressure is made as deep as one can at the inner canthus, the spectrum stands about 30° on the outside of the point to which the axis is directed ; and generally speaking, I find that whatever the position of the axis when the pressure is made round the ball of the eye and within the edge of the orbit, the spectra usually appear round the margin of the field of view. I noticed a curious circumstance in making these examinations, which is, that though motion of the blunt point on the eyeball produces very free motions of the spectrum, yet, when pressure is made on any point, as the inner canthus, for instance, so as to produce a spectrum, and the point is then kept perfectly motionless, and the axis of the eye is directed inwards or outwards, which one would suppose amounted to the same thing as movement of the point itself, in this case the spectrum remains almost perfectly at rest. If the axis of the eye is directed up or down, in the same circumstances, there is a movement of the spectrum, which, however, is not at all proportional to the movement of the axis. Though these facts may seem incapable of explanation, and even inconsistent with each other, they evidently give no support to the notion that the retina has the property of representing objects in lines perpendicular to its surface.

Assuming this law of visible direction as true, Sir D. Brewster endeavours to account for the stability of objects which occupy the field of vision during the motions of the eye, by supposing that the centre of visible direction, or the point through which all the lines of visible direction pass, is coincident with the centre of motion of the eye. He says (*Treatise on Optics*, p. 224), “ when we move the eyeball, by means of its own muscles, through its whole range of 110° , every point of an object

within the area of the visible field, either of distinct or indistinct vision, remains absolutely fixed ; and this arises from the immobility of the centre of visible direction, and consequently of the lines of visible direction joining that centre and every point in the visible field." That objects within the visible field remain fixed during motions of the eye, is a fact ; but that the above is the cause of their stability, is by no means true. He says further, " Had the centre of visible direction been out of the centre of the eyeball, this perfect stability of vision could not have occurred." It certainly could not, if the centre of motion coincided with the centre of the eye, and that the retina had the property stated ; but the stability of objects, as I shall presently shew, is perfectly compatible with other properties of the retina : in fact, the supposition of the existence of the property stated, is the only thing that could make a difficulty about it. He says further (same page), " If we press the eye with the finger, we alter the spherical form of the surface of the retina ; we consequently alter the direction of lines perpendicular to it, and also the centre where these lines meet, so that the direction of visible objects should be changed by pressure, as we find them to be." Now this is a complete misapprehension of a fact ; the directions of visible objects are changed, in this instance, not from any change in the form of the eye, but because it is either directly pushed or drawn out of its direction towards the object, by actions induced in some of its muscles by the pressure ; for precisely the same change of situation will be produced, even more perfectly, by laying hold of the under or upper eyelid, and drawing the eye in different directions by means of its attachment to the conjunctiva ; a proceeding which cannot give rise to any alteration in its sphericity.

The following observations will lead, as it appears to me, if not to clearer and more simple, at least to truer notions on the subject of vision, than those usually received :—In the first place, as the refraction of the humours of the eye is unchangeable, it follows that rays standing at the same angle from the axis will always be refracted to the same part of the retina ; they will not one time come to a certain point, and another time be bent more deeply into the eye, but will always strike the membrane at the same distance from the extremity of the visual axis. Considering, therefore, the nervous matter of the retina as made up of numerous zones distributed in parallel bands around the

point where the visual axis strikes the back of the eye, we see that rays entering the eye at an angle of 45° for instance, with the axis, will fall on a zone of nervous matter situated somewhere about 63° from this point. Rays from every object standing at that angle from the axis all around, must fall upon some part of this zone; no rays from objects standing at other angles can ever touch it; and we find that this zone has the property, when rays fall on it, no matter with what obliquity, of representing the object from which they come as standing 45° from the axis. It must be remembered that I am now speaking of facts: that the retina has this property cannot be disputed, however it may be supposed to have come by it; whether it was an original endowment of the membrane, or an acquired property, is another question, one which seems to me to have lost its interest, and not to be of much importance; yet I would remark, that the circumstance of our being at length obliged, as I feel we are, to resolve all our knowledge of vision, not into a general law but into a simple statement of the fact, that all parts of the retina have the property, no matter with what obliquity the rays fall on them, of representing the objects from which they come in their true directions, is a strong argument for the latter supposition; for this is precisely what we should expect to occur as the result of experience. Taking each of the other zones of the retina in the same manner, we see that our perception of the angular distance of every object from the axis is predetermined by the zone of nervous matter on which rays from it fall, and its actual position by the part of the zone to which they are brought. Without speculating on the cause of these properties at all, it is evident that under this arrangement there can be no instability of objects in the field of view during motion, wherever we suppose the centre of motion to be situated, and whatever point the centre of visible direction, if any such there be, may occupy. In fact, as I have said before, the conferring on the retina a property of representing all objects in the direction of lines perpendicular to the surface on which the rays impinge, is the only thing that could endanger their stability during motions of the eye, since, in this case, the coincidence of the centre of visible direction with the centre of motion and centre of curvature, would be absolutely essential to its maintenance, which conditions would be by no means necessary in any other case. A little consideration will show this to be a fact.

There is something so attractive in the simplicity with which a general law groups and classifies various kinds of facts, that we are apt to be dazzled and caught by it, and to overlook difficulties which, if traced to their consequences, would certainly compel us to its rejection. I cannot otherwise account for the circumstance than a passage in the fifth edition of the late Dr. Turner's *Chemistry*, which I fell upon while writing this paper, should be so entirely disregarded by Sir David Brewster, Mr. Mayo, and many other physiologists, who, notwithstanding its clearness, still adhere to the explanation I have been endeavouring to refute, and which seems so wholly untenable. In that passage Dr. Turner expresses himself not satisfied with Sir D. Brewster's statements as to the law of visible direction, and traces its operation to consequences very analogous to those which I have exhibited above. I cannot help expressing some surprise that consequences so obvious as these should have escaped so many; the drawing even of a single diagram of the eye, if the circumstances are at all attended to, would inevitably lead to them, for it will be found in every case except where the object is in the axis of the vision, that the law usually adopted would represent it out of its true position, and the line of visible direction will not even, as Sir D. Brewster says, be parallel to the line of its true position. Even when situated in the axis, it will, if it is of any size, be magnified and distorted. Mr. Mayo, indeed, seems to have met with some difficulty of this kind in drawing the diagrams for the fourth edition of his *Physiology* (1837); for finding, on trial, that lines drawn from the point of the retina on which the rays fell, towards the extremities of the object, were not perpendicular to the retina, he very strangely, instead of questioning the truth of the law, seems to be obliged to modify his expressions regarding these "lines of visible direction," and says, in proceeding from a consideration of the simple lens and luminous point, to a diagram of the eye—"Let me substitute for this diagram one of the eye in vision, in which the dotted lines, A B, C D, are *meant* to be vertical to the points A and C, and *are to be understood* to be so." It seems curious that so accurate an observer as Mr. Mayo usually is, should not have followed this difficulty into all its consequences. Had he done so he must necessarily have arrived at the same conviction that Dr. Turner expressed so clearly, nearly four years since, and that I have been endeavouring to establish in this paper.

From the slight difference in refractive power that exists between the aqueous, crystalline, and vitreous humours, the influence of the two last on a ray of light is very trifling, so that, as I have mentioned above, by far the greater part of the whole refraction takes place at the cornea. Though this is pretty generally known, I am afraid the small amount of the refraction is scarcely sufficiently considered. Indeed, when once this first refraction has been effected, the course of the ray afterwards is so nearly that of a straight line, that a superficial observation would lead one to think that no allowance had been made for the remaining refractions. A most important practical inference arises from this ; that is, the immense importance of preserving the original shape of the cornea in operations for the extraction of cataract. Those who are familiar with the grinding of lenses and specula, and who know what exceedingly minute errors of figure are capable not only of giving rise to indistinctness, but of destroying all distinctness in the image, will perceive the importance of this remark. The loss of the lens must not be considered the greatest injury the eye has sustained in this case ; indeed, I strongly suspect that the uncorrected spherical aberration of the cornea is the principal source of the indistinctness that remains even with the use of glasses, after those operations for cataract, even of the most successful kind, in which the cornea has been untouched. The aberration of lenses being imperceptible in small refractions, and the refraction of the lens being exceedingly slight, in consequence of the slight difference in refractive power between it and the aqueous humour, it can scarcely be supposed, if the index of refraction given be at all near the truth, that its peculiar structure of diminishing density was introduced for the purpose of correcting its own aberration, which could never be worth taking into account.

I have never seen, nor even heard, of an operation for cataract, in which perfect distinctness was produced ; yet this ought sometimes to take place in operations for soft cataract, if the cornea contained a means of correcting its own aberration, and the only injury the eye had sustained was its being made somewhat presbyopic from the loss of the lens ; for a glass lens would, in such a case, make up for the deficiency of refraction, without introducing any perceptible aberration. It is therefore of great importance not to introduce any incorrigible, or unsymmetrical aberration, by any carelessness about the shape of the cornea in

the management after the operation. The great indistinctness which such unsymmetrical refraction may occasion in an eye otherwise good, is fully shown by Professor Airy's observations on the defective figure of the cornea of his own eye, the discovery of the nature of which, and the success of the remedy applied, form one of the most interesting instances on record of the application of optical knowledge to the removal of imperfect vision. I do not make these remarks from any apprehension that the replacing of the flap after extraction, or the accurate healing of the wound, could ever be considered matters of light moment, but simply from this circumstance, that when an end is sought to be attained, it is always of consequence to hold clearly in view the most important principle upon which its attainment is desirable.

While engaged in writing the above, Professor Rainy, of Glasgow, directed my attention to a paper on the same subject, which appeared in the ninth number of Poggendorf's *Annalen*, by Professor Mile. I understand that he also denies the existence of the law I have been discussing; but I have not yet been able to procure an account of his facts or reasonings on it, which I am very anxious to do.

I have seen several articles in the medical periodicals, many of them containing very ingenious speculations, on single and double vision, and other subjects relating to optical physiology, by Mr. T. Williams, Mr. Grove Berry, Dr. Graves, of Dublin, and others. All these gentlemen, as far as I can perceive, found their reasonings on these subjects on the supposition of the truth of Sir D. Brewster's law of visible direction; most of their opinions, therefore, do not admit of discussion while this remains unsettled. If they doubt the assertions I have made above, with regard to the refraction of the rays, all I have to request is, that they will take the trouble to draw a large diagram of the eye, using the refractions and curvatures given above, and inform me what part of the retina rays at the angles above mentioned really do fall on. This is the great and fundamental point, and we cannot proceed a single step until it has been fully determined. I do not contend that my determinations of these points are perfectly accurate, but I have no hesitation in saying, that they do not contain any error which, if removed, would make Sir David Brewster's law at all possible. Dr. Graves, in one of the numbers of the Dublin Journal of Science, in speaking of this law, expresses some surprise that the Rev. H. Lloyd, and Sir J.

Hersehel, should, in their works, have taken so little notice of it. I confess I was myself surprised as their being so little influenced by views which were stated so universally, and seemed to be supported by various facts ; but though I still think many of their views regarding vision incorrect, a particular examination of the subject has made me feel the necessity of every law being based upon principles that cannot be shaken, before one can insist on its universal reception.

Before proceeding to another part of the subject of vision, I must mention a curious fact regarding the intolerance of light, which I discovered some time ago. If we look at a bright sunny road in the height of summer, or at one of those white fleecy clouds called *cumuli*, the light is so intense, that, besides the pupils being contracted to the utmost, we are obliged to cover a considerable portion of them by half closing the lids. In these circumstances the sensation of intolerance is felt in the eye, and may be thought to have its seat in the retina. If, however, we close one eye entirely, we shall find that the other may be then freely opened without uneasiness, which shews that the real seat of the sensation must be some part of the sensorium itself, and not the retina, which is actually then receiving more light than before. We have here, therefore, a highly intellectual sense—intellectual as regards its anatomical connexion with portions of the brain devoted to the process of thought, and intellectual as regards the mental processes which many of its perceptions imply ; exhibiting, at least as far as concerns its common sensibility to light, the same law which has been found to prevail in other parts of the nervous system—namely, that when a certain state is induced at the centre of the nervous mass, the resulting sensation is referred to its extremity. This curious fact may, perhaps, be of some importance in the management of those annoying and intractable forms of ophthalmia, in which intolerance of light is so prominent a symptom.

The following experiments were undertaken for the purpose of determining the situation and size of the *punctum cæcum* of the retina. The greater number of them were performed in the following manner :—The back of the head being placed in contact with one wall of the apartment, the distance was measured, as near as possible, from the centre of the eye to the opposite wall. A candle was so placed as to make its image appear in the centre of a convex mirror hung there, which gave the flame of the candle a small and star-like appearance, better adapted to the

experiment. The right eye being then fixed first on the image, was directed to the left of it, and at the last point, where I was certain I could see it, a wafer was placed on the wall. Moving the eye still to the left, a wafer was placed again on the wall, at the first point, where I was certain I could *not* see it; going on still to the left, a wafer was placed at the last point, where I was certain I could *not* see it, and again at the first, where I was certain I *could*. Drawing a line now, from half the distance between the inner wafers to half the distance between the outer, it is evident that this line might be taken to represent the angular breadth of the insensible spot; and, accordingly, when the right eye was directed to the middle point of this line, the image of the candle was perfectly invisible, from its then falling on the centre of the blind spot. Moving the eye upwards and downwards from the middle of this line, the vertical diameter of the spot was obtained in the same manner. The length of these diameters being measured, as well as the distance from the centre of the mirror to the point where they crossed, the lengths thus obtained were divided by the distance of the centre of the eye, which gave the tangents of the angle subtended by the blind spot, and of its angular distance from the visual axis. As it is not necessary to go into these calculations, I have just subjoined the results. That I might not be misled in repetitions of the experiment, by my having taken the measures of those that I first performed, the experiments were varied in different ways as to distance and light; and with the same view, the calculations were not entered upon for any until the whole were completed.

FIRST SET (RIGHT EYE.)				SECOND SET (LEFT EYE.)			
No. of Experiments.	Distance from Visual Axis.	Diameter.	Centre of Spot above Visual Axis.	No. of Experiments.	Distance from Visual Axis.	Diameter.	Centre of Spot above Visual Axis.
1	15° 4'	7° 11'	1° 10'	1	15° 24'	7° 31'	1° 47'
2	15° 44'	7° 31'	(1° 18')	2	15° 28'	7° 31'	(1° 18')
3	15° 6'	6° 59'	1° 29'	3	15° 50'	6° 19'	1° 37'
4	16° 6'	6° 12'	(1° 5')	4	15° 49'	6° 19'	1° 38'
5	15° 11'	3° 15'		5	16° 18'	6° 0'	(1° 5')
				6	15° 30'	2° 45'	
Means	15° 26'		1° 15'	Means	15° 43'		1° 29'

Taking the means, we have, therefore, for the distance of the spot from the visual axis in the right eye, 15° 26', and in the

left $15^{\circ} 43'$. Taking the mean of these means, we have $15^{\circ} 34'$ as the most probable value from these experiments. But it would not be fair to take a mean of the diameters of the blind spot in the same manner, for the differences which the tables shew seem to depend upon circumstances which I will now explain, and which lead to a conclusion of some importance. Some of the experiments were performed in the way I have just described; some by placing a circular paper, seven or eight inches in diameter, on a light-coloured wall, and standing just so near it that the whole would be completely but barely hidden, when the axis of the eye was turned in a proper direction; others, again, were performed by shading the flame of a candle with a cylinder of dark paper, in which a small hole was cut for its light to appear; the experiment, in other respects, being proceeded with as at first described.—Lastly, they were done in Dr. Young's manner, with two unshaded candles. Taking, therefore, for each eye a mean of those experiments in which all the circumstances were alike, and distinguishing the others, we have the following interesting results:—

RIGHT EYE.		LEFT EYE.	
Nature of Experiment.	Diameter of Spot.	Nature of Experiment.	Diameter of Spot.
With paper on light coloured wall	$7^{\circ} 31'$	With paper on light coloured wall	$7^{\circ} 31'$
With image in mirror	$7^{\circ} 5'$	With image in mirror	$6^{\circ} 19'$
With luminous point through the cylinder	$6^{\circ} 12'$	With luminous point through the cylinder	$6^{\circ} 0'$
With unshaded candles	$3^{\circ} 15'$	With unshaded candles	$2^{\circ} 45'$

Here we have first a white paper on a light-coloured wall, in which the light is feeble and the contrast slight, we have, therefore, a large diameter. Next we see the image in a convex mirror, in which more than half the light is dispersed and lost: there is still, therefore, a tolerably large diameter. Next we have the direct light of the candle seen through a small aperture, by which the intensity of the light is twice as great as before, and we have a diminished diameter; and lastly, we have unshaded candles (Dr. Young's method), in which, besides having light of the same intensity, we have a considerably increased quantity of it; and in this case the diameter is less than in any other. Hence we see that the diameter of the spot is diminished

as the strength of the light increases ; and this circumstance seems to indicate at once the cause of the blindness which appears to owe its origin not as Mayo and others suppose, to the presence of the artery in the centre of this spot, but to the thickness of the nervous matter at this part ; the optic nerve not having yet spread out into those thin filaments which are exhibited in the structure of the other parts of the retina. This conclusion best explains the facts ; for we see, that at some distance from the centre of the optic nerve, its sensibility seems dull to moderate lights, and it is only capable of being roused by very strong lights at the centre itself. Indeed I found the centre perfectly insensible to the image in the mirror, when it was directly brought opposite it. The optic nerves, in this respect, resemble other nerves in the body which are not fit for their functions until they have been distributed in thin and fine filaments. Moreover, this conclusion, if true, seems to be important in another point of view ; for if the thickness of the nervous matter here is the cause of the blindness, it may reasonably be asked whether thinning down of the nervous matter which takes place, according to some anatomists, around the so-called foramen of Soemmering, may not be the cause of the great acuteness of vision that exists at the end of optic axis ?—supposing it should be found, by experiments of the above kind and by measurements of the eye, that these two parts coincide. I found the presence of the artery at the centre of the optic nerve quite perceptible by a reddish glare, which shewed itself about the centre of the invisible part of the field ; but this appearance only took place in the experiment with unshaded candles.

In performing the above experiments, I found a tendency in the eye, in moving outwards, to move also a little upwards ; from the circumstance that the image, or candle, was more perfectly hidden then, than when the axis of the eye was directed to a point in the same horizontal line with it. This seemed to indicate that the punctum cæcum was situated a little higher than the extremity of the visual axis. It is evident, however, that no complete proof of this could be obtained, except from experiments performed with both eyes at the same time, since there is otherwise nothing to assure us that the head is not placed obliquely during the experiment ; which would not much matter in the case of both eyes, as one would be depressed as much as the other was elevated, and we could take a mean. The

results of the experiments performed with both eyes at the same time, are given in the third column, and included in brackets. They represent the centre of the optic nerve as elevated above the plane passing through the visual axes of both eyes ; in one experiment $1^{\circ} 18'$, and in another, $1^{\circ} 5'$. I say the plane containing the axes of both eyes, because it is evident that many planes might contain the axis of one eye. Taking the mean of these values, we have $1^{\circ} 11'$ as the probable elevation, from experiments on both eyes at the same time. I have not, however, discarded the others, as they are uniform in indicating an elevation, though it may not be, and is not probably correct in its amount.

In most of the experiments with the mirror, the vertical was somewhat longer than the horizontal diameter. I had reason to think, however, that this arose from the flame of the candle being longer in the vertical than the horizontal direction ; and when I made use of the cylinder of paper, with a round hole in the side, to expose a part only of the flame, this difference at once disappeared.

Experiments such as the above, if repeated carefully, would give a means of determining the part of the retina on which the visual axis rests, or whether it coincides with the symmetrical axis, or what is the amount of its deviation from it if it does not. M. Le Cat considered the diameter of the insensible spot to be about one-third or one-fourth of a line. Daniel Bernouilli found it to be about one-seventh part of the diameter of the eye, and Dr. Young made it about 5° ; while my experiments, performed in the same manner with candles, represent it as about 3° . Such differences are of less importance when considered with reference to the varying circumstances mentioned above, on which they depend. I dare say a small point of light, of exceeding intensity, would assign a very small diameter to the insensible portion of the retina, if it was capable of discovering it at all.

PROBLEM VI.

BY WILLIAM GRIFFIN, M.D.

IS LARYNGISMUS STRIDULUS, OR THE CROWING DISEASE, A SPASMODIC OR PARALYTIC AFFECTION ?

BEFORE entering on the discussion proposed in the foregoing problem which has been suggested by the ingenious and clever work of the late Dr. Ley, it may be of use to give a sketch of the few cases of laryngismus stridulus which first attracted my attention to the subject.

The complaint does not appear to be so remarkably rare of occurrence, as was imagined when it first came to be accurately described by medical writers. The cases I shall relate all happened within the last two years in my own practice, and the great majority of the profession who have written on the diseases of infants latterly, evince a familiar acquaintance with it. The fact that an affection, I might almost say so common, should have been completely overlooked by almost all the eminent men of past times, is sufficiently mortifying, whether looked upon as illustrative of the difficulty of the diagnosis, or a general inaccuracy of observation.

I shall first describe two cases in which the phenomena of the crowing disease formed but a minor or less important part of the whole affection. They are curious as illustrations of a complaint, which, I believe, has not even yet been noticed by any medical writer, and which might, perhaps, to distinguish it from the crowing disease, to be appropriately called the *crowing apoplexy of infants*.

An infant of rather a spare and puny frame, although healthy looking in its countenance, on the eleventh day after birth, was affected with bowel complaint. The motions were very fluid; of a light yellow colour, and there were about six in the course of the day. It got a little chalk powder from the nurse at night, and next morning a drop of laudanum. The motions were less frequent, though still loose, and the child did not look well. It had been fed on milk and water, and a little prepared barley from its birth, the flatness of its mother's nipples preventing its obtaining much nourishment at the breast; and it was now suckled by a woman whose child was twelve months' old, but

perfectly healthy, while they were waiting to procure a younger nurse. In the evening, however, it was seized suddenly, as if with suffocation, losing its breath, and becoming first pallid, afterwards dark or purplish in the face, and finally, when all respiration was suspended, of a death-like hue. There was a stiffening of the frame and twitching about the mouth, and the thumbs were drawn into the palms of the hands; but there was no convulsion of the body or extremities. After a longer or shorter interval the breath was recovered by gasps, which were accompanied by a crowing sound, that became louder after a time, so as sometimes nearly to resemble hiccup. This crowing again gradually died away, and the respiration became easier, but always recurred on the approach of another fit, and continued until the breath was lost, when the child, as in the first instance, grew dark for a moment or two, and then pale and death-like. It was indeed sometimes impossible in these intervals of suspended respiration, which recurred frequently, to say whether the child was living or dead. It lay cold, white, breathless, and without sign of animation, often for three or nearly four minutes, counted by a watch, and then recovered with a faint gasp, followed by the crowing. The intervals between the fits of suspended respiration were seldom longer than half an hour, although they occasionally extended to an hour or more. The crowing sometimes continued throughout the interval, at others abated for a little. From the first moment of the attack the infant never recovered its consciousness; the pupils seemed fixed the eyes senseless, and all power of deglutition was lost. Whenever a teaspoonful of liquid was given it remained in the mouth or flowed out at the corners, or if it went back to the larynx it obstructed the breathing, and brought on the fit of suspended respiration. The only approach to sensibility observable at any time was in the slight motion of the lips, which were sometimes seen to work as in the action of sucking. There was rarely, when the fit of suffocation commenced, a slight convulsion or twitching of the muscles of the face, which, however, never lasted longer than the blackness or darkness, and seemed in fact a struggling for breath; but as the case became protracted, although the crowing in the intervals continued as loud as before, no darkening of the face or writhing of the features preceded the fit.

As the power of swallowing was gone, the treatment was confined to injections, stimulating liniments to the spine and stomach, and the warm bath. The first consisted of starch and assafoetida. Turpentine was afterwards used, and when these means seemed unavailing, laudanum was administered. The injections were sometimes retained for half an hour, but usually came away soon, and in no case had any perceptible influence in preventing the fits, although as much as eight drops of laudanum were given in this way. The bath at first seemed of service, and prolonged the interval, but after some repetitions lost its effect. A blister, which was applied to the back of the neck, was equally fruitless, and after an illness of about forty hours' duration, the poor little sufferer ceased to breathe any more.

CASE II.—A fine, round-limbed, healthy-looking boy was born after a favourable labour of about six hours' duration. He got castor oil, as infants usually do, and on the morning after I was requested by the nurse to inspect the motions, which were very green; the one she showed me had the appearance of chopped spinach. I directed a grain of calomel to be given, and if it did not move the bowels freely, the oil in the morning. Next day the motions still continuing green, though less so than before, the calomel was repeated. On the fourth day the bowels were much better, and on the fifth the evacuations seemed to be of the ordinary character. The child during this time got nothing except the breast, and (the mother not being able to nurse long nor often on account of sore nipples) a little prepared barley, or milk and water. It appeared to thrive, and did not lose its plumpness, but the mother afterwards informed me it used to start frequently with a cry or scream out of its sleep, in a way she had never observed with her first child, who was alive and well. After this sudden start or scream it usually fell asleep immediately. On the evening of the tenth day, when my attendance had terminated, the child was attacked with slight complaint in the bowels, for which it got a little chalk mixture. The bowels were moved five or six times during the night, not very profusely; and the evacuations were of a pale yellow colour. At eight o'clock the next morning I was summoned hastily to visit it, and on entering the room, to my utter consternation, I found it gasping, after a fit of breathlessness, precisely similar to that described in the former case, and followed by the same crowing noise in inspiration. It was perfectly insensible; the

pupils of the eyes were natural in their appearance, but sluggish in their movements; the power of swallowing was gone; the thumbs were bent into the palms of the hands; the surface was pale and cold; the impulse of the heart and the pulse at the wrist were feeble. The fit of suspended respiration occurred at intervals of perhaps twenty minutes, though they were sometimes longer. The child's countenance did not darken in the fit as in the other case; it became instantly pale on losing its breath; the lips and even the tongue were cold, and when I put back my finger to the pharynx, to ascertain whether I could by the touch excite the action of swallowing, I found the parts motionless. The crowing continued almost through the whole of the interval, and, if it did cease, it always recurred on the approach of the fit. On one occasion, after the warm bath, it seemed to subside into a breathing slightly stertorous, which did not at all occur in the case of the former child. There was, however, once or twice, in the course of the day, the same working or sucking motion of the lips which I noticed in that case, indicating some faint approach to sensibility.

The treatment of this little patient differed in no respect from that employed for the former infant, except that no laudanum was administered, and it was equally unsuccessful. If it could be said that anything gave the least relief, it was the warm bath. A blister to the nape of the neck and vertex was applied early, but before it could have had any effect, the little sufferer expired, having struggled altogether only six hours against the disease.

On examining the body of the first infant no appearance of disease was observable in the heart, lungs, or bowels. On opening the head there was excessive difficulty in detaching the skull from the dura mater, and as soon as it was removed the hemispheres fell asunder in a disfluent or pultaceous mass, so that it was impossible to make any regular examination.—Having removed the gelatinous mass of brain and cerebellum, the medulla oblongata and spinal cord were found of healthy consistence.

I now thought I had a clue to the explanation of the symptoms. There appeared to be here sufficient disorganization to account at least for the unconsciousness and attach probability to the supposition that the crowing respiration was dependent on some affection of the brain. Billard speaks of such general ramollissement, as often occurring immediately after birth, and

mentions that it is then more considerable and extensive than at any other period of life. He thinks it probable that it sometimes begins even before birth. I do not know what the attending symptoms were, but in ten such instances in which the softening extended to the whole of the spinal cord, he relates, that the respiration was laborious and imperfect, the limbs flaccid and motionless, and the pulsations of the heart scarcely perceptible. These symptoms were all absent in the case of the infant which I have detailed, it would seem, because there was no lesion or disorganization of the spinal cord or medulla oblongata; but the functions of the hemispheres and of the cerebellum were altogether suspended, as the ramollissement of these parts would lead one to expect,

There could, however, be no stronger proof, that identity of functional derangement is no evidence of the identity of the lesion which produces it, than appeared on examining the body of the second infant, the symptoms of whose disease so closely resembled those of the first. The brain seemed perfectly healthy, and there was no sign of disease in any other organ that I could detect. The examination in either case was a very hurried one, which precluded any examination of the state of the eighth pair of nerves, or recurrents, or of the bronchial glands. Indeed it did not occur to me as a matter of any importance to make the examination at the time, as I thought it very improbable there could be any scrofulous enlargement of these parts immediately after birth. There certainly was no perceptible enlargement of the glandulæ conæatinatæ, or other glands in the neck. The thoracic and abdominal viscera were also in this case healthy. I do not know, however, what importance, if any, to attach to one circumstance: the cardiac orifice of the stomach was found plugged up with a firm coagulum of milk, which retained the exact shape of the parts when it was removed.

The complaint which I have described as affecting these two infants, though closely allied to laryngismus stridulus, or crowing disease, is obviously very distinct in its nature, or more truly perhaps in the amount of nervous matter involved in the morbid action. Possibly the same difference may exist between them, as between apoplexy and local convulsion, or, as Dr. Ley suggests, palsy. Whatever it may be, the distinction between them is very marked. In laryngismus there is no insensibility, no crowing, except immediately on recovering from the fits of breath-

lessness ; no apparent illness whatsoever in the intervals, no pallor or coldness of the surface, or feebleness of the action of the heart, or of the pulse at the wrist. In the affections which I have just described, on the other hand, there was, from the moment of seizure, an utter unconsciousness and insensibility both in the fit and interval, so much so indeed that even a cry did not escape either of the infants, except in one instance, when the first was plunged into a hot bath ; there was an incapability of swallowing, excessive feebleness of the heart's action, and coldness of the surface of the body. In the second child, there was at one time, when the crowing subsided during a longer interval than usual, some approach to stertor in the breathing. I ought perhaps to notice one other distinction between this and the crowing disease of Dr. Clarke and others, that it did not terminate in general convulsions as the latter usually does.

It is exceedingly difficult even to speculate with any probability on the cause of this remarkable affection, or to connect it with any certain pathological condition. From its occurrence a few days after birth, at which period, Billard states, general *ramollissement* of the nervous centres is most common, I should have been disposed to connect the symptoms with some such disorganization, if, in the second case, the brain had not been found apparently healthy ; and I should have attributed them to some congenital defect, which the weak appearance of the other infant might in some degree countenance, only that no suspicion of the kind could be entertained in reference to the second. It was born a strong, plump, round-limbed little fellow, in every way as promising for long life as one could desire. It might, indeed, be said that the very earliest evacuations were unhealthy, and the sudden screams out of sleep, described by the mother, indicated something wrong from the commencement ; but these symptoms are of every day occurrence with other infants who go on well notwithstanding.

From a fair consideration of both cases it becomes a question of great interest, whether the complaint might not be one of mere functional derangement, as we are well assured many cases of fatal convulsions are. It was in both instances preceded by disorder of the bowels, and on the evening previous to the attack by diarrhœa. Supposing it to be a functional affection, to what cause are we to attribute it ? An experienced practitioner informed me he had met with similar cases which also proved

fatal, and that he believed them to depend upon retention of the meconium. In the first case narrated, however, castor oil was given to the infant immediately after birth in the usual manner, and repeated as occasion seemed to require; and in the second, calomel and castor oil were given until the evacuations assumed a natural appearance. Could the hard plug of coagulated milk, found in the cardiac orifice of the stomach of this child, by possibility produce such a frightful affection of the nervous centres?

I now proceed to the cases in which the question at issue between a portion of the profession and the late Dr. Ley is more directly involved. They accurately agree with those already published by that gentleman and others, and are given in detail only because the amount of those already upon record is far too slight too admit of safe general inferences as to their pathology or treatment. In all diseases, the treatment of which is difficult or obscure, the numerical system of induction so successfully adopted by Louis in affections of the lungs, is the only one worth our attention, and without a large number of cases this cannot be resorted to, even in the imperfect manner, which these, when given loosely by different individuals, may admit of.

A fine, stout, muscular little fellow, at the age of seven months was seized, in the nurse's arms, with sudden suspension of the breath, but after a slight struggle, and gasping attempts at inspiration, accompanied by a crowing sound, he in a few moments recovered. As the fits recurred two or three times in the course of the day, and occasioned a very just alarm to the parents, I was requested to see the child, whom I found playful and smiling on the nurse's knee before me. From the description of the fit, as it was called, I had no doubt as to its nature. My acquaintance with it having been chiefly derived from Dr. Clarke's communications on the subject, I entertained his views of its cerebral origin, believing that in this case the brain had become affected from the irritation of teething in a naturally plethoric habit. The gums were therefore lanced, active purgatives were given; and the child, who was a great feeder, was restricted to a lighter diet. The complaint recurred slightly for some days, when one or two teeth appearing, it ceased altogether, and for five or six weeks the child continued perfectly well. It was then, however, attacked with a more violent fit than before. As well as I can recollect (for I kept no notes of the case) I then lanced the gums again, blistered behind the ears, and

directed some purgatives. The complaint recurred frequently when the little fellow laughed or cried, and sometimes he awoke out of sleep with the gasping and crowing. He passed some days entirely without an attack ; on others he had one or two. I now directed assafoetida and antispasmodics for him ; but as there was no satisfactory amendment, the fits still returning occasionally in a very alarming manner, a consultation was proposed with a physician of eminence and experience. This gentleman took altogether a different view from mine of the disorder, and I saw evidently that he had either never seen it, or never distinguished it as a specific affection. He spoke of the crowing as singultus, and attributing it to acidity of stomach, with derangement of bowels, recommended that milk and its other usual diet should be laid aside, and broth substituted ; a carminative mixture with rhubarb and magnesia was also directed. There was, however, no improvement, and on the second day after the child fell into convulsions. The face was very much distorted, and the convulsions were confined to one side, the other appearing to be palsied. Three leeches were applied to the temples, and the child was put into a warm bath. After a second consultation four leeches more were applied, and the little patient at length recovered, but was hemiplegic. The paralytic affection, however, disappeared, after the application of a blister to the nape of the neck and vertex. The improvement, though wonderful, was however temporary ; the fits of crowing returned on the next day, and in two or three days, the little sufferer fell into convulsions again, followed by with profound coma, from which he never recovered.

The mother had another child in the following year, which fell into convulsions in, I believe, the second month, and died. I did not see it, but mention the circumstance to show the family predisposition to such affections.

Some months after my attendance on the foregoing case, I was sent for by a lady who had lost many children, to prescribe for an infant that had been just seized with convulsions. She had only this little one of seven or eight months old, and another, a weak, emaciated boy aged four or five years, who was suffering with paralysis of the lower limbs from spinal disease out of a family of eight children, most of whom had died of convulsions or hydrocephalus. The child was in a warm bath, and recovered from the fit when I had arrived. It was like the former, of a gross habit, and though still at the breast fed largely.

It took bread and milk often, was allowed broths, and sucked meat greedily. It certainly did credit to the diet, for it was one of the finest children I ever saw, large, round, plump, and rosy faced, with eyes full of light and intelligence, and a disposition full of play. I found it had been attacked in a similar way about three weeks or a month before, and that a scabby eruption, with which the whole head and upper part of the face had been covered was then first observed to decline. It had been teething for some time, and used to dribble a great deal, but this drain of saliva had also lately diminished. The convulsions, as described to me, were general. As the child was very plethoric, three leeches were applied to the head, the gums were lanced, he was freely purged, and small blisters were applied behind the ears. On the following day, although there was no return of the convulsions, he had once or twice, as the attendants described, a slight fit, though without spasms of the limbs. A more minute inquiry convinced me that it was an attack of the crowing disease. The child, they said, stiffened, and lost its breath; and although the crowing did not strike them particularly, when I imitated the sound and gasping manner of the infant, they at once recognized the perfect resemblance. I believe, indeed, one reason for the apparent infrequency of this disease is, that it is always spoken of as a convulsive fit by the nurse or attendants, and as the physician is seldom in time to witness the paroxysm, unless he institutes an accurate inquiry, and obtains a faithful and minute description of the attack, he necessarily confounds it with the common convulsions of infants. I was, indeed, for several days in attendance on this little boy, during which he had several returns of the fit, before I had an opportunity of verifying my conclusions, although from the mother's faithful picture of the fit, after her attention was directed to the importance of discrimination, I was perfectly convinced on the subject. The blisters were now kept open with savine ointment; the bowels, which were exceedingly costive and obstinate, were regularly opened; and he was restricted to liquid and farinaceous diet. In less than a week he seemed perfectly well.

On calling to see the child in about a fortnight after, I found him much pulled down; his face pale, his look dull and depressed and he had lost his cheerfulness; there had been, however, no return of the crowing. Somewhat apprehensive of the effects of the debility which had followed the regular purging and

restricted diet, I again allowed the little fellow broth, with some bread or panada ; in a few days he began to look up again, and before a week his former bright looks and playfulness had returned. About two months after this I was once more summoned to see him, and to my great regret found this fine boy had relapsed into a worse condition than he had been in any former attack ; the gasping and breathlessness recurred frequently, and were very protracted ; and the recovery of the breath, with the loud crowing, took place only after most distressing struggles. He was still teething, his bowels were very confined, and I lamented to see that the eruption on the head and face had nearly disappeared. The same treatment as before was once more resorted to ; lancing the gums ; the warm bath ; repeated doses of active purgatives, and enemata ; leeches were also again applied—these last remedies chiefly in consequence of the very plethoric habit of the child. When a sufficient degree of depletion had taken place, the fits still recurring at intervals of two or three hours, or oftener, blisters were applied behind the ears, and injections with assafoetida were administered. On the next day a blister was applied to the neck, and a few drops of laudanum were added to each enema. Other antispasmodics were also administered by the mouth, but without evincing any influence in arresting the frequent attacks of the complaint.—The laudanum was given two or three times in the injections, and did not produce any effect whatsoever, upon which I did not press it further. The little patient passed on the whole a better night than the previous one, but on the succeeding day suffered much ; the attack occurred from the merest trifle, and was attended with appearances of convulsion. Towards noon he appeared stupid and heavy, and the pupils more dilated and sluggish in their movements. As the pulse was quick and feeble, the face pale, and the skin cold, I now gave some wine, and fed the little fellow occasionally with broth. An astonishing improvement followed the use of the wine, such, indeed, as I have not unfrequently witnessed in the advanced stage of hydrocephalus, when the great debility seemed to demand it, but invariably without leading to any permanent good. The insensibility and disposition to fall into a state of coma completely disappeared, the little sufferer looked up again with evident consciousness, and the fits of suspended respiration were shorter

and less distressing. In two or three hours after, however, when I repeated my visit, I found all matters worse ; the respiration was loud, and tracheal, and he seemed again insensible, though still recovering some degree of consciousness whenever the difficulty of breathing occurred. In this utterly hopeless state I was obliged to leave him to visit another patient who was in imminent danger, and learned afterwards that he died towards morning, convulsed.

The only symptom of convulsion which I witnessed throughout was the contraction of the thumbs in the palms of the hands.—The first attack, the one with which the child suffered a fortnight or three weeks before I was called to see him, and that on the day I commenced my attendance, which, however, I was not in time to witness, were both, I believe, instances of common convulsion. I had not read Dr. Ley's papers on this disease until a day or two previous to the boy's death, when I was induced to examine the state of the lymphatic glands particularly. *I found the glandulæ concatenatæ enlarged all down the neck*, but did not obtain permission for a *post mortem* examination.

CASE III.—I met with another case some time after equally interesting. The mother of a fine little boy, aged nine months, during my attendance on its father, almost casually mentioned to me that it had had a strange crowing noise in its breathing once or twice in the night, losing its breath at each time of its occurrence, and blackening up for a few moments in a very strange manner. She mentioned that it had this crowing since it was four months old, occurring chiefly when it was awakened out of its sleep, or when it became impatient for food and was not instantly gratified. Believing it to be merely the result of passion or quickness of temper, its continuance ever since more or less did not excite any uneasiness in the minds of the parents, until the suspended respiration and darkening of the face supervened. The mother was even then surprised at my alarm about the nature of the attack and my expressions of apprehension that it would terminate in convulsions. The child had had some disturbance of bowels for some days, and was passing greenish or colourless gelatinous stools, perhaps to the amount of four or five in the day, not very liquid nor discharged with irritation. As there appeared to be one or two teeth pushing up the gums were lanced and a grain of calomel with a grain of James's powder was ordered every third hour. It had one or

two fits in the course of the day, and in the evening fell into convulsions which subsided on its being got into a warm bath.—As the bowels had been freely moved by the calomel and the gelatinous stools still continued, a warm oatmeal tea injection with a little assafoetida was ordered, which however was not retained a moment, and a powder of mercury with chalk and a little Dover's powder was given followed by occasional doses of the milk of assafoetida with some rhubarb and magnesia. At midnight another convulsion fit occurred, which was again relieved by the warm bath. The breath had been suspended in the intervals followed by blackening of the features several times. Although there was neither heat of skin nor thirst, nor flushing of the face or appearance of fulness of the circulation in the head, as a measure of precaution I directed a leech to be applied behind each ear, after which the application of blisters.—A grain and a half of calomel was ordered every second hour, an enema with seven drops of laudanum, and a warm poultice to the abdomen. There was no tenderness there, but I observed that the stools were always preceded by symptoms of pain or uneasiness and forcing, upon which the crowing fit instantly and invariably supervened—the instant rejection of all enemata also argued some degree of irritation, although there was no tenderness upon pressure. It occurred to me that if the warm cataplasms and the opiate enema (should it remain) happened to allay the uneasiness of bowels, the fits might be prevented altogether. In case of failure I left a phial of laudanum with the mother, and desired her to give from four to seven drops every hour until the gelatinous stools and uneasiness of bowels was allayed. The enema, though repeated two or three times, was, as I apprehended, returned back before the pipe was withdrawn, and as none of the other remedies produced the slightest benefit, the fits still recurring frequently, four drops of laudanum were given by the mouth—then five—then seven, after which the child shewed some signs of drowsiness—all irritation and uneasiness of bowels ceased, and this improvement was still more strongly marked by a total cessation of the fits either of crowing or convulsion. On visiting the infant next morning I found it lying quiet, breathing easily and regularly with a less rapid pulse, cool skin and moist tongue—it took its drink well which it could not do in the night, and made its accustomed little cooing playful noise on getting it—the last stool was of a very improved

character, and the child's eagerness for feeding shewed evident hunger. It was always rather a full feeder, getting usually three large cups of bread and milk every day, but since the occurrence of the attack I had restricted it to whey and milk and water.—We were now in great hopes that the little patient's life was comparatively safe, at least as regarded the present attack. It passed through the day exceedingly well, was perfectly sensible, and was even animated and almost playful in its looks when fed with whey or milk and slept quietly at intervals—the bowels were moved once or twice with improved appearances, and there was no symptom either of uneasiness of bowels or crowing. When, however, the mother was changing the little fellow's clothes before the fire at night she fancied its eyes looked sunk, and that altogether it was not so well. She fed it however and it took its drink well, but in half an hour or an hour afterwards the extremities began to grow cold, the whole surface of the body pale and clammy, and the countenance sunk and inanimate; I was instantly sent for, and on arriving found my little patient hopelessly gone—the pulse almost imperceptible and in other respects resembling an infant in the collapse of cholera. There had been no evacuations of any kind to account for this—no vomiting—no diarrhœa—no perspirations. Even the two motions which took place in the whole day were more natural and moderate—not greater in fact than any of those which occurred when five or six occurred in the twenty-four hours. The child was still sensible, taking its drink eagerly and stretching its little hand for the spoon, though the power of swallowing was evidently becoming more difficult. It expired in about two hours afterwards without pain or struggle.

From the commencement of this case, whenever the crowing fit occurred, it was accompanied by the usual incurvation or contraction of the toes and fingers—there were altogether but two or three convulsion fits. The parents lost a child before by hydrocephalus, and another had slight obliquity of vision, the result of some cerebral affection.

On examination of the body in twenty-four hours after death, we found the sutures completely open, and generally over the whole cranium a deficiency of ossification—so great indeed, that that the frontal, parietal, and occipital bones were readily divided with the scalpel—the veins ramifying on the surface of the brain appeared to be turgid, but it was throughout its whole extent

perfectly healthy in its structure as far as we could ascertain.—The only appearance that we could consider morbid was the proportion of water in the ventricles which amounted to about six drachms. The abdominal viscera were also examined, without our being able to detect either in the liver or large or small intestines any indubitable marks of disease—the latter were for the most part pale and somewhat distended with air.

A fourth case happened in the practice of my brother, who has given me the following account of it. “ In the year 1835 M. W., a child a year old, began to be affected with derangement of bowels, white tongue, loss of appetite, and diarrhœa, attended with thin, gruel-like discharges of a pale colour; these attacks, which were at first attributed to difficult dentition, were usually got over in a week or ten days, but their repeated occurrence produced a degree of delicacy of look, and softness of fibre, and very much checked her growth. In the autumn of that year, being at the sea side, she got so severe an attack of diarrhœa, that for a day or two her life was despaired of; she came home in a very delicate state, with pallid look, white tongue, pale-coloured alvine discharges, which were now, however, of a healthy consistence, and she had lost her relish for all food except milk and thin gruel. In this state she was attacked with nervous startings, and also occasionally with what the mother called ‘croupy breathing,’ (*laryngismus stridulus* of Dr. Ley;) this last symptom was never so violent as to suspend the breathing entirely, or cause much distress. It was in other respects the loud sonorous inspiration so well described by Dr. Ley, always sudden in its onset, and passing off entirely in a minute or two. This state of things lasted some time, and during the continuance of it she was attacked with convulsions, which came on frequently during a day and a night, and which, after she had been blistered and purged, were at length subdued by an enema with fifteen drops of laudanum repeated once or twice. As the nervous startings, the ‘croupy breathing,’ and the convulsions, were all considered connected with that irritable and over-sensitive state of the nerves which is often conjoined with debility, the occasional use of a sedative with tonics was recommended, and (a tonic mixture, with some tinct. opii, having been found to disagree, by interfering with the effect of the necessary opening medicine) the sedative was usually administered in the form of an enema, with fifteen drops of laudanum,

whenever the startings or 'croupy breathing' shewed themselves. Under this plan, and by attention to the bowels, she passed six months without any return of the convulsions, and was rarely affected with the starting and croupy breathing, which were always watched, and when they did occur subdued by the enema with tincture of opium. She had a very capricious appetite all this time, the tongue usually coated or white, and for a considerable period she would take nothing but bread soaked in wine, on which regimen she gradually, however, gained some flesh and strength. Calling at the house one morning, I found her bowels had been rather confined for some days before, and she had a fit of the croupy breathing in my presence, though not a very violent one. When it was over, and the child was quiet again, the mother was expressing her apprehensions that this was premonitory of a convulsive attack, and asked me if I thought so. I gave an answer tending to quiet her fears, but had scarcely done speaking when the child had a violent convulsive fit, which came on without any of the croupy breathing, and lasted nearly a minute. I put a blister on the nape of the neck, ordered some opening medicine immediately, and remained in the house three hours, during which time she had no return either of croupy breathing or convulsion. I was obliged to go eighteen or twenty miles off that day, and on my return on the next, at noon, found the family in the utmost distress, the convulsions had returned repeatedly during the night, though the bowels had been well moved, and the blister had risen. The fits were growing more violent and very frequent, returning every half hour or twenty minutes. I applied three leeches to the temples, gave the laudanum enema, and applied the stimulating dressing to the blister; the enema was repeated at intervals of about an hour, being usually given whenever a fit occurred of more than ordinary violence. After it was given five or six times it had an evident effect on the paroxysms, which though not lessened in frequency were very much diminished in violence, and indeed were now very slight. The child at this time had a very exhausted appearance, her face was pale, her pulse rapid, and there was so much insensibility that she could scarcely be got to swallow the drink which was occasionally put between her lips with a spoon. The insensibility was perhaps due to the opiate enemata, but as the convulsive paroxysms still returned, though not so violently, and the comatose state was one to which the

case naturally tended, though no opiate had been given, I feared if it was allowed to continue, and that my inference proved correct, the time lost would be irretrievable. The child was already blistered, and as new blisters would be too slow of acting, it occurred to me as an alternative to make an opening along the sagittal suture, down to the bone, and place a dossil of lint in it, wet with spirits of turpentine : she showed some sensibility to this by movements of the head from side to side ; there was some little tendency to the fits for two or three hours afterwards, but they were so slight as scarcely to attract notice. The insensibility wore gradually off, and by the next evening she was able to take some milk and gruel, and recovered from all the effects of the attack, except the debility.

“ The child lived eight months after this attack, but in an extremely delicate condition ; her growth was checked ; she showed a disposition to rickets in the limbs ; acquired a double lateral curvature of the spine, and though nearly three years of age could neither speak nor walk. During this interval she had occasionally, but very rarely, returns of the croupy breathing in a slight degree, and sometimes returns of the nervous startings, which symptoms were always attended to and subdued in the usual manner when they did occur. I could never perceive in this child any appearance of swelled glands in any part of the neck during her illness, nor any where else except beneath the occiput, where some small ones could sometimes be felt, produced, probably by the discharge from the head, which was kept up to the time of her death. This event occurred after an attack of diarrhoea and vomiting, much slighter in degree than many that she had had previously. There was no examination of the body.”

As the intimate pathology of these cases is likely I fear for a length of time to remain a mystery, it becomes a matter of extreme importance to ascertain in how far the pathology of the nerves in general and the inferences deduced from analogy may determine the general question whether they belong to the class of paralytic affections, and are dependent on the pressure of the recurrent nerves by lymphatic glands ; or to convulsive disorders, and arise from some change occurring at the origin of the par vagum, affecting chiefly the superior laryngeal branches. The late Dr. Hugh Ley has already in a very elaborate work considered this subject in all its relations. He has indeed

brought such a mass of information together in support of his views, and reasoned so ingeniously, that he has I believe made converts of a great many of the profession to his opinions. There are, after all, difficulties which Dr. Ley has by no means satisfactorily got over, and which are sufficient to throw doubts on his whole hypothesis.

The physiology of the parts engaged in the disease, and many facts illustrative of the general pathology of the nerves, give an extraordinary speciousness to Dr. Ley's views, which we shall not find so fully supported on a more minute examination. I shall, however, first consider what value should be attached to one or two symptoms, the presence of which, in the majority of cases, seems undeniable, and with respect to one at least is, in Dr. Ley's opinion, necessary as the exciting cause of the malady ; I mean eruptions on the scalp, ears, or face, and enlargement of the lymphatic glands in the neck or thorax. When we reflect how rarely the most alarming symptoms of the disorder, the suspension of respiration and crowing, are found in connexion with disease of the head, which is so common ; how impossible it is to account for that infrequency on any supposition of the affection originating in disease either of the superior laryngeal nerve, or of the origin of the par vagum ; and how perfectly it seems to be explained on the opposite view, as arising from pressure on the recurrent branches by enlarged lymphatic glands —when further we find that most cases are attended by eruptions about the scalp, ears, or face, with enlarged glands just in the course of the recurrents ; when we know that injurious pressure may possibly occur, the effect of which must be, diminished power in the nerve ; and lastly when we are aware that the consequences of such defective power or paralysis must be difficult if not impossible respiration, we must admit that a very specious case is made out in favour of the only hypothesis which offers a solution of the difficulties involved in the pathology of laryngismus stridulus. It is, however, worth while to examine the separate links of this imposing chain of reasoning.

It must strike every observer with regard to the enlarged glands, that as they are very common in children, as common almost as the disposition to struma, the crowing disease should be common also. Dr. Ley anticipates this objection, but he puts it only as applying to the immunity of adults from such an affection, which he accounts for by saying : 1st, adults are compa-

ratively free from the causes which produce such enlargements ; 2ndly, where the trachea has acquired the adult size, it gives more protection to the recurrent ; and 3rdly, the larynx and glottis of the adult are much larger than those of the infant. These arguments, strong as they are in explaining why such an affection should rarely occur in an adult, by no means satisfy us as to his perfect immunity : amidst all the tumours to which adults are subject about the neck, it is rational, if Dr. Ley's hypothesis be true, to demand the citation of at least one fair case of laryngismus stridulus arising like it, resembling it, and terminating as it does in the infant. The real difficulty, however, is not about adults but about grown children, between whom and mere infants there can exist no extraordinary disproportion in the size of the trachea, larynx, or glottis ; and in whom the disposition to enlargement of the lymphatic glands is, I should say, rather greater. Why are not grown children attacked with laryngismus stridulus ? I have never seen the complaint except in mere infants ; all Mr. Robertson's and most of Dr. Ley's cases appear to have been under two years of age ; and Dr. Underwood speaks of it under the head of inward fits, as one of the disorders of early infancy.

The mere presence of enlarged glands in this complaint, may certainly, as Mr. Robertson of Manchester suggests, be a coincidence only. Dr. Marsh asserts that he has never seen the crowing disease in any but infants of strumous habits, and with such, enlarged glands are usual, whether they suffer from any such affection or not. Strumous ophthalmia is very frequently attended by enlarged glands, and disappears as the enlargement subsides, yet it furnishes no argument of the dependance of the one on the other, both being the result of a common taint in the system. Enlarged glands are indeed, apt to appear with most strumous diseases of infants. Mr. Swan states that he has frequently found the glands within the chest enlarged in children who died of hydrocephalus, yet I question whether there was any such phenomenon as crowing in a single one of these cases.

Mr. Robertson very fairly inquires, " is it probable, that in a soft yielding structure like the throat, absorbent glands should, by pressure on the par vagum and recurrent nerves, cause a diminution or extinction of nervous energy ? " If the pressure could be supposed sufficient, no one could doubt the effect ; the question for consideration is, can it be so ? The only satisfac-

tory instances of such effects from such pressure adduced by Dr. Ley, are those cited from Sir Charles Bell's work, in which partial paralysis of the face occurred from the pressure of an enlarged gland on the seventh nerve between the mastoid process and angle of the jaw. Even in these, however, the analogy is not perfect, as one may readily conceive that an equal degree of pressure would occasion far more important effects in that situation, than in the lower and less resisting parts of the throat. If the pressure of enlarged glands could in the latter situations so readily affect the energy of the nerves, it is wholly inconceivable that it should not more frequently happen in strumous children with enlarged glands; and admitting for a moment, the assumption that the effects of the pressure would be immediate, it seems extraordinary that it should not often take place in those violent strainings or twistings of the neck, which in their games and exercises so continually occur. This last observation, I have made, because Dr. Ley states, in explaining the occasional absence of the crowing, "that glands not constantly in contact with a nerve may, during the varied contractions of the muscles of the neck, in crying, coughing, laughing, or sudden twisting, produce at once contact and compression, *when temporary asphyxia will be the result.*" I do not, however, believe, that momentary pressure on a nerve, however great, occasions any inconvenience at all, of which we have every day evidence in our own persons. To impair the energy of a nerve, the pressure must be continued for some time, and when once impaired, it is a considerable time before it is again recovered, although the pressure be removed.

This brings us to the consideration of the paroxysmal nature of the disease, which Dr. Ley acknowledges he has always looked upon as the strongest argument for its spasmodic character.—Adopting Mr. Swan's explanation of the paroxysmal nature of all convulsive and painful disorders of nerves, quoted by Dr. Ley, "a nerve cannot at first bear a diseased action without rest, any more than a healthy one, and therefore the diseased action after a certain period, ceases to make any impression; but after this rest, the nerve acquires fresh powers, and is again fitted for the same action. In palsy, on the other hand, the pressure being permanent, the loss of sense or motion, or of both, is permanent also." If this were universally true, it would be altogether fatal to Dr. Ley's supposition of the paralytic nature of laryngismus stridulus, as he would find it impossible to reconcile the rapid recovery, easy respiration, and long intervals of

perfect relief between fits occurring on the same day, with permanent defect of power. But it is only true, he says, as far as the nerves of sensation and volition are concerned; for in paralytic ailments of the muscles supplied by respiratory nerves, the palsy, though continuous, is not constantly manifest; and in proof of this, he instances the effect of pressure on the portio dura, the defective power occasioned by which is not observed, till in speaking or in emotion, or difficulty of respiration, vigorous action of the muscles is required. So in like manner he says, in the glottis "the effects of the enfeebled, if not paralyzed, state of its opening muscles, are only observable in those more vigorous efforts which are made when the respiration is hurried or impeded, as in fright, fits of anger, sudden awaking from sleep, in consequence of some external impression, and in screaming, crying, coughing, &c."

It appears to me, that the whole question of the spasmodic or paralytic nature of the disease turns chiefly upon the correctness of this distinction which Dr. Ley has drawn between the voluntary and respiratory nerves. To take the instance which he has himself put forward in illustration, that of the seventh nerve, he has certainly fallen into error, probably in consequence of having seen no case in which it was perfectly palsied. In perfect or even in very considerable palsy of the portio dura, I have always seen the face permanently dragged to one side, whether the muscles were in active contraction or not. In slighter cases, where the energy of the nerve is not much impaired, I am aware the defective power is not observable until the opposing muscles are thrown into vigorous action, but this would be equally true, if the opposing muscles were supplied by purely voluntary nerves. The law that if the pressure is permanent, the defect of power must be permanent exactly in the same degree, is in fact true of the portio dura, as it is of all other nerves; and referring this law to the glottis in paralysis of the recurrents from pressure, whatever effect is produced, and in whatever degree, must be as permanent as the pressure that occasioned it. I do not here take into account the greater defect of power observable in the palsied muscles in one side of the face, when the opposing ones are in vigorous action, or those of the eyelid when we attempt to close it, or in the glottis when the closing muscles are spasmodically or energetically contracted. In these instances the effect arises from active contraction of the healthy muscles, not solely from the palsy of the others, and therefore can last only as long

as muscular action usually lasts—a few moments. But even this effect, in its constant recurrence, if not in its continuousness, bears a permanent relation to the paralytic affection, since no action of the unaffected muscles can take place without its appearing. It is clear, therefore, that if we are to explain the paroxysmal nature of the crowing disease, by any such effects of loss of power in the recurrent nerves, we must assume that when once the paralysis is more or less imperfectly established, whatever ill result or difficulty of breathing is at any time observable in laughing, crying, screaming, fright, &c., should invariably return with every recurrence of these, and a certain amount of mischief, however slight, should be always distinguishable when at perfect rest; just as the slightest defect of power in the *portio dura* is observable in the slightly parted eyelid while the patient sleeps. But the fact seems entirely otherwise; children affected with this disease pass days without the slightest appearance of illness, between two fits of suspended or difficult respiration, during which they laugh and cry, and are excited as usual.

The foregoing observations must sufficiently display the error Dr. Ley has fallen into with regard to the effects of palsy of the recurrent nerves in occasioning the phenomena of laryngismus stridulus. On the whole, after all the consideration I have devoted to the complaint, and having, I think, given other ingenious arguments of Dr. Ley their full weight, I must still confess myself a disciple of the older doctrine, that the affection is one of spasm or partial convulsion like cramp, rather than of paralysis. The fact of its being frequently benefited by antispasmodics, with which Dr. Underwood tells us he latterly cured most cases, and by anodynes, as opium, hemlock, cicuta, &c., recommended by all modern writers on the disease, favours this view; the circumstance of the sudden occurrence of the gasping and crowing on washing with cold water, laughing, crying, or agitation of mind, also supports it, as well as the almost universal coexistence of the carpo-pedal contractions, and the frequent termination of the complaint in convulsions. But above all these, as a strong analogical evidence for its spasmodic character, I place its paroxysmal nature, and the manner in which the paroxysms occur. The office of the superior laryngeal nerves would lead us to expect a disposition to spasmodic action on the least irritation or excitement, recurring at irregular intervals, dependent of course on the return of the irritation or excitement,

but far more on the increase or decrease of the susceptibility of the parts, and disposition to spasmodic action. Dr. Mayo very justly remarks, that "a frequent disorder in parts endowed with acute sensibility, like the mucous surface of the larynx or eyelids, is an increased susceptibility of the sentient surface, and a tendency to spasmodic action in the adjacent muscles, which usually act from impressions received upon it. Thus in the urethra a morbidly sensible state of a part of the mucous membrane produces spasmodic stricture, or a continued contraction of the surrounding fibres of the accelerator urine. In like manner is produced spasm of the glottis, in cases of ulcer within the larynx and in hydrophobia." The muscles supplied by the superior laryngeal nerves are the sentinels of the chink of the glottis, as the orbicularis is of the chink or opening of the eyelids, and their action is directed by an exquisite sensibility of the parts, which is at once manifested on the attempted entrance of anything injurious to the lungs, whether it be a drop of water, or a volume of mephitic gas. One can well understand how dangerous any morbid increase of the sensibility of such nerves at their extremities, or any existence of irritation at their origin, might prove; why the danger should occur in irregular paroxysm; and why the exciting cause which occasioned them on one day should be altogether powerless on the next. If it be enquired further, why such a dangerous result as the suspension of respiration in the crouping disease does not then occur more frequently, it can only be replied, that we are wholly ignorant of the morbid condition which disorders the functions of those nerves, or whether it exists at their extremities, or their origin in the medulla oblongata. If we suppose the affection to be organic, we should find it more difficult to account for the occasional recoveries under very mild treatment, than the usual fatality under the most active. If it be functional, and therefore symptomatic, we can better understand why it might depend on a variety of causes; at one time upon an affection of the head, at another of the bowels, at another upon dentition; we can comprehend, too, how these several affections, influencing peculiar predispositions, may in one child occasion hydrocephalus, in another convulsions, in a fourth, that more rare infantile disorder, the crouping disease.

This seems to be very much the view taken of the affection by

Dr. Marsh, and more lately by Dr. Stokes. The disease as the latter physician observes may shew itself as a simple spasmodic affection of the larynx, *independent of any other perceptible lesion* ; but this is the rarest case. In others, it is connected *with the irritation of dentition or deranged digestive functions* ; while in a third class, it is symptomatic of primary cerebral disease. In this last the spasm of the glottis is as symptomatic of the cerebral disease as the convulsions of the extremities.

On the treatment I have but little to offer. The obvious preliminary to a judicious plan of cure is to ascertain, by a careful examination of all the functions of the body, whether there be anything wrong to which we could refer the origin of the disease. When any such causes or complications, whether in the head or abdomen, are found to exist, their removal should form our first object, but always with reference to the strength or tone of the system, which in an affection invariably found in connexion with a strumous habit, cannot be lowered much without injury. The maintenance of the general health and strength seems to be on all occasions a matter of importance, but still more so in those cases in which no complication exists, and where probably the complaint very much depends on debility, in connexion with either a cachetic state of the body and wasting, or preternatural irritability with plethora. From my experience of the complaint I am disposed to believe that no treatment, directed entirely with reference to its complications, or on the supposition of its being a pure cerebral affection, can be successful, and I fully agree with those practitioners whose great anxiety is to restore the natural tone and firmness of the system, giving tonics where there is deficiency of power, and antispasmodics and anodynes where there is great irritability. To attain our object, however, it must always be held in mind that in the chronic affections of children, the exhibition of medicines is secondary to the management of diet and change of air. The former should be regulated with the utmost care, and in many cases Dr. Marsh's suggestion with respect to a succession of good nurses during the whole period of dentition might be attended with advantage. Change of air not only in this complaint but in all obscure chronic cases is invaluable, being in fact the only remedial measure about the beneficial influence of which there can be no question, however our views of the nature or pathology of the disease may alter from

time to time. I have often thought that in such complaints, and still more in those which are supposed to be endemie, it would be most desirable, in every instance where it was at all practicable, to treat the patient in an atmosphere differing as much as possible from that in which his disease originated. I believe many cases of eroup and cholera, such at least as are not of the rapid type, might be treated successfully if removed into a new atmosphere, which are utterly hopeless when the cure is attempted in the locality where the patients sickened. Whether a child affected with laryngismus have change of air or not, however, it is essential that he shall sleep in a cool and airy apartment, and that when the weather permits he shall be as much as possible in the open air. There is no one point in which children are more mismanaged than in the arrangement of their sleeping apartments, which are commonly the most close and confined in the whole house.

Dr. Merriman has recommended the use of continued purgatives, so as to procure at least two free motions daily. When the child is plethoric, or is otherwise likely to bear evacuations well it would, I have no doubt, be an advisable plan; indeed in young children the bowels are usually moved as often as this in the twenty-four hours with advantage, but the recommendation, in any case, should be followed with constant regard to the strength of the little patient, and the apparent effect. He has also advised the use of soda or burned sponge, probably with the same view which induced Dr. Ley to propose the preparations of iodine, to diminish the size of the enlarged glands in the neck and chest, to the pressure of some of which on the recurrent nerves he attributed the disease. Doubtful as I am of the correctness of this opinion, I should think it judicious to adopt the treatment, especially as it could in no sense interfere with other remedial measures on which I should perhaps set more value. It seems to be a fair indication to endeavour as much as possible to counteract or overcome the general strumous disposition in those affections, which are seldom or never found to occur except in conjunction with it.

I have offered these few unsatisfactory observations without wishing to attach more importance to them than they merit. I believe both the pathology and treatment of the disease are still very uncertain, and that it will require all the consideration

and inquiry which observant practitioners can bestow upon the subject for many future years to attain a just knowledge of either. The following summary of the amount of our present information and of the facts connected with the disease, may be useful to subsequent inquirers.

By the concurrent testimony of almost all who have noticed the affection, it occurs for the most part, if not wholly, in strumous habits.

It is frequently found in connexion with enlarged glands in the neck, and perhaps in the thorax.

It is frequently found in connexion with eruptions on the face, ears, or scalp.

It frequently terminates in convulsions, and is sometimes, though very rarely, ushered in by them.

It is met with in families in which children are subject to head affections or convulsions, but who have also the strumous disposition.

It is sometimes met with in connexion with an apoplectic or comatose state from the commencement, as in the cases of crowing apoplexy which I have described.

In a great proportion of the cases which terminated fatally there was no symptom of head affection through their whole course, beyond the occasional fits of breathlessness and crowing, and the children were as well apparently, a few moments before death, as they were previous to the first attack of the disease, or as any children could be.

The complaint is sometimes, but rarely, attended by cough or permanent difficulty of respiration.

I believe it may be said that far more than half of all the cases of which we have any account, terminated in death.